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Year: 1978

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## On the necessity of “appropriate behavior” on the part of the caregiver

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DOI: <https://doi.org/10.1017/s0140525x0007583x>

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ZORA URL: <https://doi.org/10.5167/uzh-154562>

Journal Article

Published Version

Originally published at:

Bischof, N (1978). On the necessity of “appropriate behavior” on the part of the caregiver. Behavioral and Brain Sciences, 1(3):438-439.

DOI: <https://doi.org/10.1017/s0140525x0007583x>

# Toward a general theory of infantile attachment: a comparative review of aspects of the social bond

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**Abstract:** This critical appraisal of contemporary interpretations in the area of infantile attachment begins with an outline of the principal features of the Bowlby-Ainsworth ethological theory, the instrumental/operant learning theory of Gewirtz, and Hoffman's classical conditioning model. Some attention is also given to Cairns's contiguity learning analysis and the Hoffman-Solomon opponent-process model. Discussion of these theories is followed by a review of representative data from infants at four phyletic levels (precocial birds, dogs, monkeys, and human beings), with an emphasis on three aspects of social bonding: (a) the formation and persistence of social ties in the infant under conditions of maltreatment, (b) the role of the attachment object in the adjustment of the infant to the broader environment (the so-called secure base effect), and (c) the infant's reaction to involuntary separation from the attachment object.

An attempt is made to judge how well each of the interpretations accounts for all or part of the data, with the conclusion that current theories do not accord completely with documented attachment phenomena. The following criticisms are highlighted: Ethological theory emphasizes that infants' behavior systems have been shaped by the ordinarily expectable environment and depend on that environment for their functioning, yet infants of many species form bonds to objects not typical in any species' environment, or even to sources of maltreatment. Learning theory is faulted for making predictions contradicted by the maltreatment data and for a lack of formal mechanisms to account for the secure base and separation effects. The contiguity analysis is criticized for its inability to account for the emergence of certain response patterns during separation, and the opponent-process model is called into question because of its failure to fit important affective dynamics of social separation (a central focus of this theory). Recommendations for future theories of attachment are offered.

**Keywords:** attachment; ethological theory; imprinting; infant; learning theory; maltreatment; opponent-process theory; secure base; social separation.

Our purpose in this paper is to appraise contemporary explanations of infantile social bonding. It is widely agreed that the infants of many vertebrate species become psychologically attached to their parents, and by all accounts this motivational process is extremely powerful. There is little agreement, however, on the nature of this process, and no consensual specification of the factors that energize and direct the occurrence of the infant's filial responses. Our aim is to evaluate alternative explanations of the psychology of infantile attachment.

Two recent developments prompt this endeavor. First, during the past three decades rigorous and systematic attempts to detail the formation of social ties between infants and specific adults have proliferated. These empirical efforts have resulted in a mapping of antecedents, features, and consequences of attachment. The fact that a fairly wide selection of species has been studied permits a considerable amount of comparison. Second, the empirical boom has been accompanied by a growing literature of interpretation. Writers of a number of persuasions have at-

tempted to account for infant sociability within the frameworks (and limitations) of particular behavioral systems.

From these dual perspectives, empirical and theoretical, the aim of the current paper is relatively straightforward. We now have a fair idea what the phenomenon of attachment looks like, and we know what various theories expect it to look like. It would seem a reasonable matter to collect the data and the theories in one place, and to assess the fit between the two. This is what we have attempted to do.

## Theories of attachment behavior

There are a variety of theories or models of the development of attachment in infants. Some of these accounts seem to fall under general rubrics such as "learning theory," "ethological theory," or "psychoanalytic theory." However, since each position is somewhat different from all others, it may be more appropriate to

discuss these ideas in relation to the individuals who formulated them, rather than by “schools of thought.” We will discuss eleven different perspectives on the formation or dynamics of attachment in infants. (The eleven interpretations we entertain by no means exhaust the range of thinking in this area. However, most other theories seem interesting only from an historical perspective. Thus, we will not take up such topics as Allport’s “functional autonomy,” Freud’s “cathexis,” McDougall’s “sentiment,” or Murphy’s “canalization.”) The first six positions, while potentially useful or important, receive little direct attention from contemporary researchers on attachment. Nevertheless, it is worth outlining these views, if only to show the diversity of thinking in this area. We will then provide a detailed critique of five contemporary positions.

### Earlier and peripheral theories

This section contains short descriptions of salient theories and estimates of the current standing or status of each of the positions. The order in which the series appears is somewhat arbitrary, but it generally indicates the chronology of the contributions.

**Lorenz’s paper on imprinting.** In 1937, Lorenz published a seminal paper, “The companion in the bird’s world,” in which he described his discovery of imprinting – the phenomenon whereby social bonds were formed in young precocial birds. Lorenz ascribed to imprinting features that seemed to identify the phenomenon as a unique mechanism in early socialization (at least in birds). Imprinting was thought to occur only within a severely limited time span, to be governed or elicited by particular stimulus configurations, and to be irreversible in that the bond could neither be undone nor replaced.

These provocative notions stimulated an enormous amount of research, the result of which is that all of Lorenz’s postulates on imprinting can be viewed as incorrect (cf. Bateson, 1966; Rajecki, 1977). Although some research continues to be devoted to these issues, most of the work in the field is focused elsewhere.

**Anna Freud, and the anaclitic origins of human attachment.** Working within the ego psychology tradition, Freud (1946) suggested that the social bonding of human infants occurred in stages, and that these steps had an anaclitic basis (i.e., they were based on the child’s dependent need to be fed). A child’s first “love” was the love of feeding, which resulted in experiences of wish-fulfillment and pleasure (narcissistic stage). As the child’s awareness developed, the child recognized the immediate source of its pleasure, and its love was shifted to the milk, breast, or bottle (transitional stage). Later, the child became aware of the ultimate source of tension reduction via feeding and then felt love for its mother or caretaker (object relation stage).

Freud’s (1946) ideas have not been widely influential, perhaps because they have been subjected to only the most preliminary sorts of tests (Décarie, 1965). Further, Ainsworth (1969) has faulted this position on comparative grounds, since infants of other species (e.g., chickens and monkeys) form social attachments independent of feeding experiences.

**The Dollard-Miller secondary reinforcement model.** Dollard and Miller (1950) pointed out that in the first year of an infant’s life there are thousands of occasions when the mother or caretaker ministers to the child’s physical needs. Therefore, there is generally a negative correlation between the presence of certain adults and periods of hunger, cold, or wetness, and a positive correlation between adult presence and the alleviation of tension states. By dispensing such primary reinforcers, a person in the infant’s environment takes on secondary reinforcing properties. The child engages in “attachment” behavior (smil-

ing, approaching) in order to gain proximity to that person and access to his or her secondary reinforcers.

Theoretically, infants should become attached to only those individuals who provide physical care, but this is not always the case in practice. A survey by Schaffer and Emerson (1964) indicated that as many as 22 percent of the babies in their sample had formed strong attachments to people who had *never* been involved in their physical care, and an additional 17 percent of the children were attached to individuals who were seldom involved in caretaking. Moreover, the secondary reinforcement model cannot account for the attachment behavior of certain nonhuman species, as noted in the preceding section. For these reasons the model does not enjoy wide acclaim.

**Schneirla’s epigenetic analysis.** An infant’s approach to a social object can be taken as an indicator of attachment. Schneirla (1959, 1965) argued that in the early stages of behavioral development, stimulus intensity (and not stimulus quality) was the key factor in eliciting approach responses. In this scheme, low (or diminishing) levels of stimulation resulted in approach, but too intense (or, presumably, increasing) levels caused withdrawal.

Tests of this notion have yielded disappointing results. Based on the model, Moltz (1963) predicted that hatchlings would be more attracted to an imprinting object that had retreated from them (thus providing diminishing stimulus intensity), rather than to one that alternately retreated and approached (where the effect of diminishing stimulus intensity would be offset by periods of increasing stimulus intensity). However, Moltz’s (1963) data failed to confirm this prediction. In light of this work and of a related study by Kovach (1970), Rajecki (1973) concluded that the epigenetic position could not claim substantial support in the domain of imprinting.

**Hess, and the critical period for imprinting.** Hess has published a series of statements (1959, 1973) in which he reiterated the claim that there is a critical period for imprinting in precocial birds (and, possibly, for the formation of attachments in other species, including human beings). While many workers in the field recognize that birds and other animals are more likely to form attachments during some periods in development than during others, Hess claims that this is so because there is a genetic mechanism that strictly (and sharply) limits critical periods for imprinting.

One of Hess’s sternest critics has been Bateson (1974), who points out that in order to make an unqualified claim for such a mechanism, “he [Hess] must either ignore or misrepresent strong evidence that the length of the period of sensitivity greatly depends on the nature of the young bird’s experiences” (p. 740). Moreover, Hess (1973) views his own work on the effects of early experience in birds as basically different from that of other workers in the area, which Bateson (1974) suggests may simply be a device for dismissing damaging evidence. In sum, Hess’s ideas on imprinting have not been universally well received, and it is difficult to judge his contribution to a general understanding of attachment.

**Scott’s learning theory of social motivation.** Scott (1971) has developed a learning theory of social motivation that is similar to the Dollard-Miller model, but in Scott’s model a key mediating factor is the parent’s absence (in addition to the parent’s presence). Scott argues that the basic mechanism in the production of social attachment is the emotional distress engendered by the absence of a familiar social object. Usually, this sort of separation is brief, and the child’s distress is relieved by the reappearance of the familiar person. Scott views the infant’s emotional reaction “as an internal reinforcing agent, punishing the infant when familiar individuals are absent and rewarding him by its disappearance as contact is re-established” (p. 230). It follows from this premise that the strength of attachment should

be some positive function of the number of separations that the infant experiences.

At least two lines of evidence argue against Scott's position. First, young monkeys show profound emotional disturbances upon their *initial* separation from social objects (Hinde & McGinnis, 1977). That is, they seem to have formed remarkably strong attachments (as indicated by the degree and duration of their reactions) without prior separation experiences. Second, if repeated separations result in the strengthening of the social bond, each successive separation should result in a reaction more traumatic than the preceding one. However, studies of the young of species such as dogs (Elliot & Scott, 1961) and chickens (Rajecki, et al., 1978b) indicate a diminution of negative emotional reactions over the course of multiple social separations. These kinds of data seem to contradict Scott's thesis.

### Current theories directly concerned with attachment

We now take up several of the more influential theories of attachment. These positions are included because they meet three important criteria. First, these models are regarded as generally useful analytic tools in accounting for what is already known about infantile attachment. Second, research is being stimulated by these ideas. Third, the models differ in focus and, therefore, each potentially offers a unique understanding. The theories are to some extent independent, and so we will evaluate each on its own terms.

The organization of the remainder of the paper is as follows. For convenience, we will retain the labels that these positions already bear. We will first outline in some detail three fairly broad approaches: the ethological theory of Bowlby and Ainsworth, Hoffman's classical conditioning model, and Gewirtz's instrumental/operant learning theory. Thereafter we will sketch two rather more focused interpretations: Cairns's contiguity analysis and the Hoffman-Solomon opponent-process model of the affective dynamics of attachment. These introductions will be followed by a review of findings on three aspects of early social bonding: maltreatment, secure base, and separation effects. (These phenomena will be described and their selection justified below.) We will review data concerning these effects at four phyletic levels: young precocial birds, puppies, infant monkeys, and human babies. Following this, we will evaluate each theory's ability to account for some or all of the maltreatment, secure base, and separation effects. The ethological and learning positions, being the most general approaches in this area, will be scrutinized in detail at the end of each section. The contiguity and opponent-process analyses, being narrower in scope, will be evaluated only where appropriate. The paper will close with a discussion of the usefulness of the contemporary theories and with recommendations for future theories of infantile attachment.

**The Bowlby-Ainsworth ethological theory.** Bowlby (1958, 1969) and Ainsworth (1969, 1972) have proposed a theory of human infant attachment that holds that infants are born with a biological predisposition to seek proximity to and contact with conspecific adults. Briefly, Bowlby proposes that at birth modern infants manifest the vestiges of behavior patterns that played a role in maintaining contact with caretakers earlier in the evolutionary history of man. These behavior patterns include reactions like the Moro (the embracing reflex), traction, and grasp reflexes. Compared with other primates, however, the ability of human infants to maintain physical contact is minimal. Among altricial human newborns, therefore, Bowlby suggests that signalling mechanisms have assumed primary importance. Infantile signals such as crying cause adults to approach and tend to the infant

emitting the signal, and later signals like babbling and smiling may entice adults to remain nearby (Frodi, et al., 1978). While motor behaviors like clinging predominate in other primates and signalling behaviors predominate in human beings, these are viewed as functionally equivalent, inasmuch as both types of behavior facilitate the maintenance of adult-infant proximity, which in turn assures the infant protection. Because they promote protection (against predation and exposure), these forms of behavior have been selected in the course of genetic evolution.

Bowlby and Ainsworth argue that adults, too, are biologically predisposed to assure proximity to and protection of the infant. Their behavior may be elicited by infant signals, but need not be. Both adults and infants thus behave in a manner destined to increase the amount of interaction the infant has with particular adults. This is important because, according to these theorists, it is the *amount* of interaction between an infant and a specific adult that determines whether an attachment will form (Ainsworth, 1973, pp. 54, 55). Bowlby (1969, p. 273) proposed four principles to explain the development of specific and discriminating attachment: (a) "an in-built bias towards looking at certain patterns in preference to others and at things that move", (b) "exposure learning, by which the familiar comes to be distinguished from the strange", (c) "an in-built bias to approach the familiar (and later to withdraw from the strange)", and (d) "feedback of results, by which a behavioural sequence is augmented when it is followed by certain results and diminished when it is followed by others."

In human beings, preferences for particular individuals are expected to emerge by three months of age, although specific attachments are not possible until the infant is cognitively capable of appreciating the permanent existence of other persons (Piaget, 1952, 1954). Although analogous attachment processes are believed to function in other species, Bowlby and Ainsworth do not explain why similar cognitive constraints do not retard the bonding of nonhuman beings.

Another ethologically oriented theorist has proposed a rather different bonding mechanism. Lamb (1976, 1978) argues that there is an evolutionally-determined set of adult prepotent responses presumed to parallel and complement the infant's behavioral repertoire. What is important for the formation of attachment, Lamb argues, is that the adult emit *appropriate* and *sensitive* responses to the infant's behavior – any simple contingent response on the part of the adult will not suffice. Certain people respond appropriately to the infant's signals more often than do others: They retrieve the infant when it cries, comfort it with physical contact rather than vocalizations, and sensitively pace their interactions with the child. It is to these people that infants become attached. Lamb (1976, 1978) derived this position from the evidence that human infants regularly become attached to their fathers as well as their mothers even when the amount of father-infant interaction is minimal.

Ainsworth (1973; Ainsworth et al., 1974) has also emphasized the importance of the appropriateness of the adult's response. She differs from Lamb, however, in her assertion that these qualitative factors do not affect whether an attachment will form but, rather, that they will influence the security of the resulting relationship. Infants whose attachments are insecure will behave somewhat maladaptively within their environment (see "Secure base effects in children," below) and would be less likely to survive in the long run.

According to Bowlby (1969, 1973) and Ainsworth (1972, 1973), specific separation protest will not occur before the third quarter-year of life. Prior to this point, protest may be interpreted as a signal to attract into proximity (and thus interaction) any adult willing to engage in the social stimulation that infants enjoy. After specific attachments are formed, however, protest should occur upon separation from only attachment figures. Protest represents the infant's response to a natural (i.e., innate) "cue to danger." That is, separation from a protective adult leads the infant to emit a signal aimed at bringing the adult back into



proximity, where its protection is once more functional. The return of the attachment figure removes the cue to danger and the distress response terminates. Prolonged separation shatters the infant's faith in the attachment figure, since its cries have been ignored. The perceived breach of faith brings a termination of protest and yields a period of grieving in the infant, who is bereft of his usual source of comfort and security. After the grieving process, the infant may develop new attachments to replace the bonds that were disrupted.

Bowlby has also incorporated into his theory the notion of a set-goal, which is based on a model formulated by Miller et al. (1960). The set-goal essentially refers to the degree of proximity (to an attachment figure) that an infant deems satisfactory. The limits of the set-goal vary, depending on a variety of internal-organismic factors (i.e., fatigue, hunger, and illness cause infants to seek greater proximity), and external factors (i.e., separation and other "natural cues to danger" lead infants to seek greater proximity). The common characteristic of these factors is their presumed relation to changing needs for protection, depending on the condition of the infant, and the degree of threat posed by the environment. Simpler behavioral systems become incorporated into a more sophisticated goal-corrected control system between about nine and eighteen months of age (Ainsworth, 1969). The concept of the secure base is related to the set-goal notion. Bowlby and Ainsworth propose that the presence of an attachment figure provides the infant with security that extends the limits of the set goal and thus permits the infant to explore and affiliate at some distance from the attachment figure.

Physical assault, regardless of the source, obviously affects the limits of the set-goal, causing the infant to seek contact with the protective attachment figures. From this there follows the counter-intuitive prediction that the infant will seek proximity to an attachment figure when distressed, even when the attachment figure (protective agent) emits the maltreatment from which the infant seeks protection.

As we understand it, the various ethological theorists make the following predictions concerning the aspects of infantile attachment that are under scrutiny.

1. Two classes of predictions are available from the ethologically oriented writers concerning the effects of maltreatment during the bonding period. The first kind of prediction is that while maltreatment would not necessarily prevent the formation of attachments, such conditions would influence either the strength or quality of the bond. Bowlby asserts that the infant's responses result in feedback that may augment or diminish those responses. He further suggests (1969, p. 273) some response-augmenting forms of feedback for the infant (such as the mother's tendency "... to gesture, to talk or sing to him, to pat or hug him"), but apparently omits examples of the forms that could serve to diminish the child's prosocial behavior. (Presumably, beating, biting, or applying painful electric shock to the infant might well serve this function.) In a related vein, Ainsworth (1973) makes the claim that the quality of the adult-child interaction influences the child's sense of security, but not the existence of the bond itself.

On the other hand, Lamb (1976, 1978) holds that the formation of the child's attachment depends on the appropriateness and sensitivity of the social object's responses. Since maltreatment is neither appropriate for the species nor sensitive to the infant's state or needs, attachment to agents of maltreatment should not occur.

2. By incorporating a notion like the set-goal, the theory has provisions for predicting that the infant can use the social object as a secure base to explore the surrounding environment. A corollary to this point is that the impact of novel or threatening environmental effects (considered apart from physical maltreatment) would be ameliorated by the presence of the social object.

3. Involuntary separation of the infant from the social object should result in a specific chain of reactions. The infant should initially show distress signals in an effort to retrieve the social

object. It would next shift into a state (grief) that reflects its recognition that the social object is not forthcoming, despite protestations. Recovery from the grief state follows, at which time the formation of a new social bond may occur.

**Hoffman's classical conditioning model.** Hoffman and his colleagues (Hoffman & DePaulo, 1977; Hoffman & Ratner, 1973) have proposed a reinforcement model of the formation of specific attachments. These writers focus on the imprinting<sup>1</sup> of ducklings, but they judge their theory to be applicable to mammals, including human and nonhuman primates. Hoffman's position is based on five premises. The first three account for the formation of the bond: (a) precocial birds have an innate disposition to respond filially to certain kinds of stimuli, such as visual movement, (b) stimuli that are capable of eliciting innate filial responses are innately reinforcing as well, and (c) the remaining features of the object (e.g., size and shape) that were originally neutral with respect to filial responses come to elicit such behavior because over time they are associated (in the Pavlovian sense) with the innately potent stimuli. The remaining premises account for maturational restrictions on the formation or expression of social ties: (d) in ducklings there is an increasing tendency (over the first few days after hatching) to respond fearfully to a novel imprinting object and (e) the responses of a naive bird to a novel social object represent a resolution of conflicting tendencies to react filially, or fearfully.

In sum, the Hoffman model is a classical conditioning model. Of course, Hoffman need not specify "positive" or "negative" reinforcers, since these concepts are not part of the classical conditioning terminology. However, since the "innately reinforcing" stimuli (Hoffman & Ratner, 1973, p. 530) elicit proximity-seeking responses (approach, following), we assume that these stimuli are regarded as pleasant, inviting, or attractive in some way. Further, "... since the initial presentation of the stimulus consistently yielded a decrease in distress vocalization, it can be concluded that the first contact with the imprinting stimulus reduces arousal" (Hoffman & DePaulo, 1977, p. 62). The assumption that the imprinting stimulus causes a reduction in arousal is crucial to Hoffman's attempt to account for the effects of aversive stimulation (usually shock) during imprinting sessions. According to Hoffman and Ratner (1973), aversive stimulation facilitates imprinting "by increasing the subject's attention and reaction to an appropriate, arousal-reducing stimulus" (p. 540). They further state that "... aversive stimulation would be expected to enhance the motivational background against which the reinforcing properties of an imprinting stimulus can operate" (p. 540).

Hoffman and Ratner (1973, p. 539) also argue that a bird that is "somewhat fearful" of a novel imprinting stimulus is more likely to direct aggressive pecks at that object than are birds whose fear is either too high or too low. Elsewhere, when referring to the relationship between fear and aggressiveness, Hoffman et al., (1974) use the phrase "optimal level for aggression" (p. 575). Because of maturational developments (see Hoffman's premise (d), p. 14), the fear of an "older" bird may be above the optimal level for aggressive responses and thus may result in competing responses such as fleeing or freezing. The fear would be less in a "younger" bird and thus, presumably, it would be nearer the optimal level for aggression. According to this view, the presence of a familiar imprinting object would have different effects on birds of different ages. The calming influence of the familiar object would *enhance* the aggressiveness of an older animal by decreasing its fear toward the optimal level, whereas it would *reduce* the aggressiveness of a younger bird by decreasing its fear to a point below the optimal level.

Hoffman's model makes the following predictions concerning aspects of infantile attachment:

1. Hoffman identifies movement as the unconditioned stimulus (reinforcer) because it seems to produce an unconditioned response of arousal-reduction. If an object (by its actions)

produces arousal in the subject, then filial responses controlled by arousal-reduction should not result, and the subject should not show evidence of attachment (imprinting) to the arousing object. We submit that an object that directly maltreats an infant would produce such arousal.

However, Hoffman states that aversive stimulation of the infant during an imprinting episode should produce heightened attachment because an enhancement of the subject's general motivational state would increase the effectiveness of the arousal-reducing reinforcer (i.e., movement).

2. Hoffman's analysis states that the presence of a familiar object would increase aggressiveness in older (fearful) birds, but decrease aggression in younger (less fearful) birds. Therefore (to use our terminology), the model predicts that secure base effects would be (up to some unspecified point) a positive function of age.

3. The classical conditioning model says nothing specific about separation effects, but since the presence or onset of an imprinting stimulus yields a decrease in distress vocalization (presumably via arousal-induction).

**Gewirtz's instrumental/operant learning theory.** Gewirtz (1972) provides a reinforcement account of the formation of specific social bonds. For him, attachment and dependence are based on the same principles; the two phenomena differ only with respect to the number of social objects involved. Dependence refers to the child's social relationship to a specifiable class of people, while attachment refers to a child's relationship to a single person. The basis for either condition is the establishment of "positive stimulus control" over the infant's behavior. That is, the characteristics of the social object "can come to function as evocative, discriminative, and/or reinforcing stimuli for the acquisition and maintenance of child behaviors" (p. 155). Dependence occurs when the child's social responses (orienting, approaching) are controlled by discriminative or reinforcing stimuli (e.g., gender, race, pattern of physical care) shared by any of a class of people. On the other hand, attachment occurs when the child's behavior is regulated by the special features (e.g., physiognomy, hair color, posture, and gait) of a particular person.

Gewirtz bases his formulation on the effects of positive (as opposed to negative) reinforcement. He mentions stimuli that are "desired" by the child, and stimuli that are involved in "caretaking and other reinforcing routines" (p. 152). More specifically, he states that "I emphasize mainly conditioning concepts for positive stimulus control over social behaviors. For heuristic simplicity, I avoid the use of fear-, anxiety-, and conflict-based concepts that have occasionally served in approaches to dependence. . . . This emphasis mainly on positive stimulus control simplifies the analysis and seems to result in the loss of little explanatory power or scope" (p. 143).

Although based on the same principles, the end-states of dependence or attachment can lead to very different behavioral outcomes, at least under certain circumstances. For example, if the child is denied access to one of its dependence objects, it can usually turn to some other member of that class to obtain reinforcement, and general behavior patterns would not be much disturbed. However, under rare circumstances "one would expect interference with response sequences . . . that initially connoted dependence . . . to be followed frequently by intense emotional responses, when there is no other person available . . . to dispense the desired stimulus or to remove the interference" (Gewirtz, 1972, p. 153). That is, the unavailability of *all* dependence-objects would result in a traumatic reaction. Similarly, if a child is separated from its attachment object, no other object would be immediately satisfactory in reinforcing the child's social behavior, and gross disruptions in general behavior patterns are predicted.

In fact, Gewirtz has several specific things to say about these "intense emotional responses" to the absence of the attachment figure. Separation from an attachment-object " . . . may often

evoke an extremely persevering, trial-and-error response pattern. When unsuccessful, the latter pattern may continue maladaptively with great ('emotional') intensity ('affect') . . . The latter outcome of interference with *attachment* behavior sequences can often suggest to an onlooker that what is at issue for the child is a response pattern involving a most important goal" (p. 163). Indeed, lengthy separations may "result in emotional patterns of high amplitude involving such responses as intense crying, rage, or undirected violence" (p. 163). On the other hand, such social separation also means that the controlling stimuli are no longer in the infant's environment, and because of this "the child's *initial* response pattern in the new situation may often be characterized as weakened or deteriorated. His responses there may even have ceased to occur entirely (leading to labels like 'apathy')" (p. 163, *italics added*).

Therefore, Gewirtz's claims would lead one to expect that, upon the involuntary separation of a child and its attachment object, the observer might well note in the child: persevering trial-and-error responses, goal-seeking responses, emotional patterns of high amplitude, including undirected violence, weakened or deteriorated response patterns, or the cessation of responding altogether! Unfortunately, Gewirtz provides neither clues as to the probabilities of these various (and, in most cases, mutually exclusive) response patterns nor any idea concerning the probable sequence of these behavioral events.

However, he does suggest why these responses would be intense, whatever their form:

"An infant's 'protests' . . . can become conditioned to cues from his mother's preparations for departures (and the short- or long-term separations that would ensue), when the mother would have responded *frequently* and rapidly to those responses of the infant. . . . Thus, also, an infant's ('plaintive') responses can become conditioned to cues from the object-person's absence, when those responses would *frequently have effected* her return to his vicinity. In such cases, the infant's reinforced responses will typically have had to bridge physical and/or distance barriers to affect the absent object-person. Therefore, his responses that come to be cued by that person's absence will often be lengthy in duration and/or intense" (p. 158, *italics added*).

This passage clearly implies that the intensity and duration of such behaviors are due to the reinforcement history of those responses. That is, in the past the "plaintive" responses have preceded the return or appearance of the social object in such a way that the separated infant will now persistently (and perhaps even more insistently) use such responses to try to effect that same outcome. In other words, this highly motivated form of behavior is due to the effects of multiple separations, which is an idea clearly reminiscent of Scott's (1971) learning theory of social motivation discussed previously.

Interestingly enough, Gewirtz (1972) is mute concerning another kind of mother-infant separation: the situation where the infant voluntarily separates itself from the parent.

In our view, Gewirtz's instrumental/operant learning theory makes certain predictions concerning the selected aspects of infantile attachment.

1. The theory predicts that the infant's social behavior is under "positive stimulus control." This seems to mean that the infant engages in prosocial behavior for the sake of positive reinforcements. If the social object dispensed (or was always associated with) punishment, maltreatment, or aversive stimulation, there would be no basis for the formation of an attachment.

2. Although Gewirtz (1972, p. 167) is aware that the infant voluntarily separates itself from, and returns to, the social object on exploratory forays, his theory has no provisions for predicting or accounting for this secure base phenomenon.

3. The theory clearly anticipates that involuntary separation will have a dramatic impact on the infant, and Gewirtz catalogues a wide variety of such reactions. However, nothing in the theory suggests the probability or sequence of these various

responses. Further, Gewirtz's formulation is heavily dependent on the assumption that multiple past separations underlie the intensity and duration of reactions to contemporary separations.

**Cairns's contiguity analysis.** Cairns (1966a) views the development of attachment as a by-product of a contiguity conditioning process. Drawing on the learning theories of W. K. Estes and E. R. Guthrie, Cairns (1966a) bases his analysis on three assumptions: (a) stimuli that precede or accompany a response become conditioned to that response, and can therefore serve as a cue for the response, (b) generally, the more salient the stimulus event, the greater its importance as a cue, and (c) such cues (eventually) contribute to the maintenance of ordered or organized behavior, and their removal would result in the disruption of orderly behavior sequences or chains. In sum, certain of the subject's response subsystems will become conditioned to an object's presence, and will be reactive to that object's absence. Thus, "at a gross descriptive level, (the subject) would appear to have formed an attachment with respect to (the object)" (Cairns, 1966a, p. 413).

Concerning such "attachment" phenomena, Cairns draws five deductions from his assumptions, four of which can serve the present explication as a list of general predictions from his analysis.

1. A subject maintained in a (mere) proximate relationship with an object will become attached to that object to some degree.
2. The more salient (conspicuous) that object, the more likely that the subject's responses will become attached (conditioned) to that object.
3. Removal of the object that has been conditioned (to serve as a cue) to many of the subject's responses will result in a disruption of those responses, and the return of the object should terminate that disruption.
4. A prolonged separation will progressively weaken the subject's attachment to the object because the subject's responses would become conditioned to other stimuli in its environment.

Finally, Cairns (1966a) notes that in his terms neither noxious nor pleasant stimulation directly influences the strength of attachment. Such stimulus properties could, however, influence attachment strength indirectly by enhancing the salience of the object.

**The Hoffman-Solomon opponent-process model for affective dynamics of attachment.** The opponent-process theory of affective dynamics (Solomon & Corbit, 1973, 1974) proposes that when some stimulus arouses a positively or negatively valenced hedonic state A (based on the primary process, a), a slave state B (based on the opponent process, b) will be produced automatically as a result of A. The b-process differs from the a-process in a number of ways. Its hedonic sign is opposite that of the a-process; latency of the onset of b is greater than that of a; b decays more slowly than does a in the absence of the original stimulus; and the b-process is strengthened by use (i.e., the duration of the A-state), whereas a is not. The b-process functions to reduce the level of intensity of the A affective state (i.e., it dampens excursions from hedonic neutrality) since that level is related to the B affective state as defined by the statement: A minus B. This opponent process system results in phenomena like adaptation or satiation, because as the magnitude of the B-state eventually approaches that of the A-state, the net affect would tend toward zero, and the original stimulus would progressively lose its capacity to evoke affect (and related responses).

When a stimulus is involuntarily and suddenly terminated, however, a rather different outcome occurs. In the absence of the stimulus, the A-state (according to the theory) decays relatively rapidly, while the B-state perseverates somewhat longer. Under these circumstances, the b-process, masked heretofore by the a-process, becomes manifest. To take the example where A is a positive or enjoyable hedonic state, the abrupt removal of the stimulus causing A would result in a negative or unpleasant experience until the B-state diminished.

Hoffman and Solomon (1974) have used the opponent-process model to account for the development of separation-induced distress vocalizations in ducklings. As we will review below, the involuntary separation of an imprinted bird from its object of attachment is usually an occasion for a great deal of agitation on the part of the segregated animal. Distress vocalizations (loud, long notes) in response to separation are more likely after longer than shorter amounts of contact with the social object, and repeated brief contacts and separations also increase the level of distress calling. Furthermore, there is evidence that a duckling placed in a strange situation will emit distress calls, but will cease doing so when a salient object is introduced. As long as the stimulus is in view, the young bird appears to be content. But if the object is suddenly removed, distress calls ensue again. Other procedures show that ducklings tested in a familiar environment seldom vocalize, but begin to do so *after* the presentation of a moving object and not before or during its presentation (Hoffman & Ratner, 1973).

These behavior patterns are interpreted by Hoffman and Solomon (1974) in the following way. For some reason (which is immaterial to the theory) an imprinting stimulus (moving object) has the capacity to evoke an hedonically positive A-state in naive ducklings. As long as the stimulus is present, the pleasantness of the A-state exceeds the unpleasantness of the slave state, B. However, upon the removal of the social object, the a-process terminates rather quickly, leaving the bird in the grip of the longer-lasting b-process. Since the hedonic value of B (and b) is opposite that of A (and a), the bird should now be relatively uncomfortable, and its distress calls are taken as a confirmation of this predicted discomfort.

The predictions of this model are not restricted to single affective episodes. Over prolonged or repeated exposure to the conditions that produce the A-state, the B-state increases until the net hedonic excursion is zero. In terms of attachment phenomena, the longer (or more often) the infant is in the presence of the attachment object, the less enjoyable would be the presence of that object, and attachment responses (approach, following, orienting) would be expected to diminish. As this particular point is not central to the issues in the present discussion, we will not pursue it further. It is worth noting, though, that it is supported by the finding that ducklings repeatedly exposed to an imprinting object eventually show a marked decline in their tendency to follow it (Moltz & Rosenblum, 1958).

Predicted changes in the B-state are relevant to the issue of involuntary separation, because the higher the B-state, the more adverse the reaction to the termination (via separation) of the A-state. As far as we can tell, the change in B is conceived of as basically *quantitative*. For example, according to Solomon and Corbit (1973), "because the later States A and B were not exactly the same as the earlier states, we labeled the later ones A' and B', respectively," and "... A' seems to be weaker than A, B' stronger than B, and longer lasting" (p. 160).

Therefore, this theory seems to predict quantitative changes in the behavior of infants during separation episodes. During any given episode there should be a diminution in the negative affective state engendered by the termination of the positive a-process. In the terms of Solomon and Corbit (1973), the sequence of events should be baseline  $\rightarrow A \rightarrow B \rightarrow$  baseline. On the other hand, over a series of separation episodes the infant should show (up to a point) progressively more severe affective reactions to forced segregation.

### Review of attachment phenomena

Several writers have already sought to analyze various attachment phenomena from a comparative perspective (Gray, 1958; Salzen, 1967; Scott, 1963). Since current attachment theorists view their formulations as applicable to more than a single species, we, too, have attempted to document certain aspects of attachment behavior at various phyletic levels in order to de-



termine how far, and in what form, these effects are found. The availability of data dictated comparisons among the following: precocial birds, dogs, monkeys, and human beings.

Next, we sought aspects of the attachment phenomenon that would allow us to contrast the theories' predictions or explanatory range. Selection here is necessary because certain aspects are not particularly useful to such an evaluation. For example, all the analyses explain the attraction of infants to attachment figures under normal or typical circumstances, but many of the relevant data antedate the theories derived to explain them. As a result, several of the purported explanations can claim the same empirical support because the theorists offer post hoc explanations, not differential predictions. Since all the theories predict (or account for) social attraction, and since social attraction indisputably exists, all theories receive support on this point. In terms of evaluating the relative merits of the positions, however, such homogeneous predictions and confirmations are undesirable.

There are, fortunately, several other aspects of the phenomenon that do not have this drawback. One of the three we will examine is the infant's reaction to maltreatment by the attachment figure (or the general abusive or aversive stimulation of the infant) during bonding episodes. Very few of the theories speak directly to this issue, but as seen, most theories rely on theoretical processes or mechanisms that may be, by implication at least, incompatible with reactions to maltreatment.

A second aspect to be considered is the fact that infants who are quite strongly attached to particular social objects will, nevertheless, voluntarily initiate temporary separations from the attachment figure. This behavior is judged to reflect an exploratory tendency, and it has been labeled the secure base effect. A corollary to this effect is that the presence of the attachment object ameliorates the infant's reaction to frightening, stressful, or otherwise disturbing influences of the nonsocial environment. Since most theories deal with the problem of why the infant directs responses to the social object itself, it will be interesting to see how these analyses fare in accounting for those occasions when the social object influences the infant's response to non-social events.

On the other hand, there are occasions when the infant is involuntarily separated from the social object, and under these circumstances the reaction of the subject certainly cannot be taken to reflect an exploratory tendency. By all accounts, the infant's experience during forced segregation is quite traumatic. While this aspect of separation effects is indeed the focus of several of the theories, it is interesting to note that the various theorists give widely differing reasons for responses to such segregation.

We feel that these aspects can be useful in evaluating the relative explanatory power of the major theories because the theories implicitly or explicitly make different predictions where these phenomena are concerned, and there are now sufficient data available to evaluate these predictions. We will proceed in the next section to review representative findings in this area. The following section is organized by a twelve-celled matrix, defined by the three phenomena under scrutiny, and infants from four phyletic levels.

## Maltreatment effects

### Maltreatment effects in birds

The influence of aversive stimulation on the formation and persistence of social bonds in precocial birds has been investigated in several ways. One general procedure involves exposing naive hatchlings to a single imprinting object under noxious conditions and measuring the birds' reaction to that object during or after exposure. Fischer (1970) exposed chicks to a moving red cube under temperature conditions that exceeded, equalled, or were lower than the birds' ambient level and found that subjects

were mostly likely to follow the object under the cold condition (see Fischer's Experiments 1 and 2). Furthermore, Fischer demonstrated the enhancement of following responses under conditions so cold as to produce continuous distress calling in the chicks (see her Experiment 4). Apparently, the only limit on this effect was that extremely low temperatures appeared to reduce the birds' capacity to move properly, independent of their inclination to follow the target. It was noted that some chicks made great efforts to follow, despite the fact that they were literally stiff-legged from the cold (Fischer, 1970, p. 417). In a related study, Rajecki et al. (1973) found that initial exposure of chicks to an imprinting target under conditions of painful cold did not preclude positive (approach) responses to the target at a later time.

The results obtained when electric shock is employed as an aversive stimulus generally parallel those obtained with cold stimuli. Kovach and Hess (1963, Experiment 2), for example, tested independent samples of chicks for the tendency to follow an imprinting object at fourteen, eighteen, and thirty-two hours after hatching. Chicks were allowed to follow the target (for a period of fifteen minutes) in the absence of electrical stimulation, or while receiving either light or heavy doses of shock. For the fourteen- and eighteen-hour age groups, not even the heaviest shock level interfered with following, while the lower pain levels somewhat enhanced following. For birds tested at thirty-two hours of age, all shock conditions interfered with following, but only the heaviest levels did so significantly. In sum, out of nine conditions involving presumably painful shock, only two produced a diminished tendency to follow, and only for older birds.

A second method in this domain again involves the noxious stimulation of naive birds in the presence of a potential attachment object, but measures of the birds' reaction to that object as well as measures of the birds' preference for objects not associated with punishment are assessed. For example, Barrett (1972, Experiment III) repeatedly exposed ducklings to an imprinting target that was always associated with shock (response-independent stimulations) and to a different object that was not associated with shock. When the ducklings were later given simultaneous locomotor choice tests (no shock involved) with the two objects, there was a striking preference for the nonshock object. However, the fact that Barrett's subjects preferred the neutral object over the one associated with shock cannot be taken as evidence that the birds were not also attached in some degree to the shock-associated target. In fact, Barrett provides interesting evidence that the ducklings did respond positively to both objects under certain circumstances. These ancillary data come from the last few shock or nonshock exposure trials that took place prior to the locomotor choice tests noted above. On these trials the stimulus was presented for several sixty-second periods, interspersed with twenty-second periods during which it was removed. During the twenty-second timeouts the ducklings began to emit distress vocalizations. On average, *both* the shock and nonshock objects caused a diminution in the rate of distress-calling, and to about the same degree (Barrett, 1972, Figure 8). Therefore, the vocalization measure failed to disclose impressive differential responsiveness in the ducklings in the face of the two types of attachment objects.

In a related vein, Ratner (1976, Experiment 1) also administered shock during the early (not initial) exposure of ducklings to a moving imprinting object and found, as did Kovach and Hess (1963), evidence that this procedure enhanced the birds' tendency to follow. In a series of subsequent locomotor choice tests, however, the effects of shock were more complicated. In the first type of simultaneous choice test, subjects were confronted with the original imprinting stimulus (a white foam rubber rectangle) and a novel "similar" stimulus (a white foam rubber rectangle bearing black stripes). In this test, control subjects approached both types of stimuli about equally, but the shocked subjects showed a clear preference for the novel stim-



ulus, a finding that seems to be in line with Barrett's (1972) finding that ducklings showed a diminished attraction toward shock-associated objects. It is possible, however, that the preference for the similar object was simply an example of a preference for "slightly novel stimulation" (Bateson, 1973) and that the shock treatment was quite irrelevant. In any event, both Ratner and Barrett have evidence that shocked ducklings were also attached to the original shock-associated objects. When Ratner gave the subjects a choice between the shock-associated imprinting stimulus and a novel "dissimilar" stimulus (a rotating beacon), all subjects, including those in the shock group, showed an overwhelming preference for the original object.

The Barrett-Ratner findings tell us, then, that schedules of electric shock can influence avian responses under certain circumstances. However, although relative preferences were affected by maltreatment, the data demonstrated that bonds formed despite maltreatment.

Researchers have also administered painful stimulation to birds already familiar with certain objects. Barrett et al. (1971) investigated the influence of response-independent and response-contingent shock on previously imprinted birds and found that following was enhanced by the former and suppressed by the latter. Of course, the simple suppression of the following response does not in itself tell us about the existence of social bonds, as noted above. Indeed, Barrett et al. (1971) provide evidence (in Experiment III) that response-contingent shock exerts only a temporary control over the filial behavior of ducklings. When ducklings are repeatedly shocked for following a familiar target, they stop following it. However, when the contingency is removed, the social responses of ducklings toward the familiar objects reemerge and return to prior levels spontaneously, that is, *without* counter-conditioning.

Furthermore, there are lines of evidence that show that precocial hatchlings react positively to potential attachment objects, even when the object itself is the agent of maltreatment. For example, Salzen (1970, Experiment 2) gave naive chicks nineteen brief exposures to a two-dimensional imprinting object: a board. On the twentieth trial, "the chick was struck sharply with the board," with the result that there was a *decrease* in the proportion of chicks showing finching, distress-calling, and withdrawal, and an *increase* in the proportion showing pleasure-calling and approach. In a second example, Melvin et al. (1967) used a live hawk as the experimental attachment object in a study of imprinting in quail chicks. They report that on initial exposure one quail was picked up and dropped by the hawk. Following the episode, the authors state that "'Shaken' but unhurt, (the subject) was inserted into the apparatus after a (two-minute) rest. The quail resumed following and showed very strong imprinting during (subsequent) sessions" (p. 237).

Finally, recent work in our laboratory (Rajecki et al., in preparation), showed that chicks subjected to large amounts of severe pummeling by the attachment object still showed prosocial reactions to that object. The objects were stuffed, cloth gloves, installed in the chicks' home cages. Once or twice daily, over several days after hatching, a chick was placed in a treatment cage (for a total of twelve separate maltreatment sessions), where an identical glove, now containing an experimenter's hand, struck the animal hard enough to knock it off its feet, or to cause it to bounce off one of the walls of the unit. Such blows were delivered every ten seconds within the twelve two and a half-minute sessions. The behavior of the maltreated chicks was compared to that of chicks in a control condition (where an inanimate stuffed glove was employed during yoked sessions), and to that of still other chicks in a "pleasant treatment" group, where the experimenter gently stroked the bird every ten seconds.

Not surprisingly, the maltreated chicks in Rajecki et al. (in preparation) were wildly disturbed during the sessions, gave many distress calls, and made vigorous attempts to escape. Also not surprising is the finding that the pleasant treatment chicks gave twice as many contentment calls during sessions as did

those that were maltreated. However, of considerable interest is the fact that the maltreated chicks gave *three times* more contentment calls (in the presence of the glove during sessions) than did the control chicks! Further, when the chicks were involuntarily separated from their gloves in the home cage, all groups showed responses typical of chicks separated from peers (Rajecki et al., 1977), indicating that attachments had formed, even in the maltreated birds.

In sum, no procedure involving pain has reliably prevented the formation (or greatly altered the existence) of some degree of attachment in precocial birds. Certain measures seem to reveal that bonding is impaired or reduced, but these effects are usually transient. Situational contingencies (such as crippling cold, or response-inhibiting shock) can certainly affect the behavior of precocial birds, but these contingencies do not disrupt the bonds themselves, even when it is the social object that dispenses the maltreatment.

### Maltreatment effects in dogs

Stanley and Elliot (1962) measured the attractiveness of human handlers in terms of how fast six-month-old basenji pups would run to them in an open field. Half the dogs were handled (stroked, nuzzled) if they approached the goal person, while the remaining pups were ignored by the goal person. Over twenty-five days of such tests the nonhandled animals showed the most social responsiveness; by the end of the experimental period the ignored puppies ran to the handlers at a speed over twice that of the pampered subjects. In a somewhat similar study (Elliott & King, 1960), human handlers fed one-month-old basenji and terrier pups, some of whom were on an ad libitum diet while others were partially starved. For the underfed pups, access to food was limited to two five-minute periods daily. As might be expected, this restricted feeding produced puppies that were chronically underweight. The procedure did not, however, produce socially unresponsive animals; in various behavioral tests it was found that the deprived group showed less avoidance and more attraction to their handler than did the control dogs.

The Stanley-Elliott and Elliott-King procedures represent studies of the effects of neglect. By contrast, Fisher (1955) attempted to inhibit the social responses of young dogs by means of strong physical punishment. Independent groups were indulged, or indulged and punished, or punished, or kept in complete social isolation for a three-month period beginning when the animals were about three weeks old. Each weekday, the indulged group received thirty minutes of permissive and friendly contact with the experimenter. The indulged-punished group were also exposed to thirty minutes of pleasant interaction with a human being, but they also encountered that same person for another thirty minutes, during which time any prosocial response on the part of the dog was punished. If the dog approached the experimenter (or, if the experimenter approached the dog), the animal received "rough handling" or "switching" until it withdrew behind a protective barrier. At other times the pup was placed on an electric grid and was coaxed toward the experimenter; if the dog approached the person it received enough shock<sup>2</sup> to produce yelping until it withdrew. The third group of pups received only the punishment just outlined; in this condition the hapless pup was invariably treated unkindly by the handler. In the fourth group the pups were reared in isolation, and had absolutely no contact with human beings from the third to the sixteenth week of life.

Fisher (1955) tested all pups for human orientation, that is, the amount of time the animal remained beside the experimenter or in contact with him. In order to show the relative impact of the various treatments, we will use Fisher's indulged group as a baseline and report the effects of the other treatments as a percentage of that baseline.

The indulged and indulged-punished pups were first tested

for human orientation late in the treatment, at twelve and thirteen weeks of age. In this test the indulged-punished group showed 231 percent of the orientation of the indulged dogs. That is, they were over twice as responsive as the control group. (This finding seems in line with the observation of Rheingold [1963] that punishment by a pup's biological mother temporarily inhibits, but by no means eliminates, the tendency to approach her.) Later, at sixteen weeks of age, all groups were tested for orientation to human beings. Again, the indulged-punished animals spent more time with the person than did the indulged group (141 percent, or a factor of 1.4). Interestingly enough, the long period of isolation did not eliminate the prosocial tendency of the isolates, who showed some 53 percent of the orientation of the baseline group. Finally, the strict punishment condition did not completely inhibit the orientation of all the dogs in that treatment. On average, these pups contacted the human being about 37 percent as much as the indulged dogs.

In sum, the data show that neglectful treatment (i.e., the absence of positive and negative treatment) and inconsistent treatment (i.e., maltreatment by and affection from the same source) both yield an accentuation of attempts to attain proximity to the attachment object. Exclusively punitive treatment, however, produces a diminished tendency to seek proximity. Puppies raised in this fashion seek proximity less than others that have been reared in isolation. One wonders, of course, why the pups in the exclusively punished group seek proximity to the handler at all.

#### Maltreatment effects in monkeys

In a seminal paper, Harlow and Zimmerman (1959) dismissed the idea that rhesus infantile attachment was based on the pleasures of feeding and argued that the surface quality of a mother (or surrogate mother) was the overriding factor in such attachment. Their notion of *contact comfort* was based on the finding that young monkeys formed a clear preference for cloth over wire surrogates, regardless of which surrogate was equipped with a feeding bottle. However, not much later it was discovered that infantile attachment was clearly evident in the face of considerable contact *discomfort*. Two lines of evidence emerged on maltreatment effects in monkeys: (a) infants' social reactions to live "motherless mothers" and (b) reactions to surrogate "evil mothers."

Seay et al. (1964) studied the maternal behavior of female monkeys that had been reared during infancy in isolation or with inanimate surrogates. These females were termed motherless mothers, and it was an unfortunate monkey that was born to such a parent. While there was considerable variety in the reactions of the motherless mothers to their infants, many responses were clearly abusive. The infants were often beaten and bitten by their mothers, and the youngsters' faces were sometimes crushed to the floor of the cage. At other times, the mothers jumped up and down on the infants. Generally, the treatment received by these babies was so bad that the authors expressed doubt concerning their survival were it not for the intervention of the laboratory personnel (see also Arling & Harlow, 1967).

Whether or not Seay et al. (1964) fully anticipated the extent of the abnormal behavior of these mothers, it seems obvious that they did not foresee the social reactions of the infants. In their paper they mention that "a surprising phenomenon was the universally persisting attempts by the infants to attach to the mother's body regardless of neglect or physical punishment" (p. 353). Similarly, Arling and Harlow (1967) note that "as was observed in the Seay et al. (1964) study, even though [the] infant [of motherless mother #27] was the victim of frequent rebuffs and violent attacks, he persisted in his attempts to gain contact with his mother during each observation session during the study" (p. 372). Apparently, there was no appreciable reduction in the attachment responses of these abused infants. In an experimental

study of social preference, Sackett et al. (1967) showed that infants raised by abusive motherless mothers "had a greater overall preference for their natural mothers than did any of the control infants" (p. 380). Further, both Rosenblum (1971b) and Kaufman (1974) have argued that the immediate consequence of (nonabusive) maternal rejection is the accentuation of proximity seeking on the part of the infant.

The effects of maternal abuse were further investigated when Harlow and his colleagues designed a variety of "evil cloth surrogate mothers" (Harlow & Harlow, 1971). In an early study, Rosenblum and Harlow (1963) constructed surrogates that periodically vented compressed air at high pressure, a stimulus condition that is extremely noxious to the infant monkey. The infants in the maltreatment condition were exposed to an average of forty-five seconds of air blast every thirty minutes. Nevertheless, the noxious stimulation failed to drive away the infants, who spent even more time clinging to the punishing surrogate ( $\bar{X}$  = 18.2 hours per day) than did control animals not exposed to air blasts ( $\bar{X}$  = 16.6 hours per day).

Following the Rosenblum-Harlow experiment, surrogates were constructed that would hurl the infant to the floor, or vibrate so violently as to make the infant's teeth chatter, or suddenly extrude brass spikes through the ventral surface, to which surface the infants typically cling). To date, apparently, none of these monsters has succeeded in disrupting the social responsiveness of rhesus infants. In fact, as Harlow and Harlow (1971, p. 206) put it, "instead of producing experimental neurosis we had achieved a technique for enhancing maternal attachment." (When allowed to choose between a warm and a cold, i.e., mildly aversive, surrogate, however, infant monkeys invariably choose the least aversive [Harlow & Suomi, 1970; Harlow et al., 1971]. Thus maltreatment does affect infant behavior when there is an alternative attachment figure, and, as in the avian literature, it is necessary to make a distinction between absolute and relative preferences for social objects.)

#### Maltreatment effects in children

For obvious reasons, there has been no experimental research on the effects of maltreatment by an attachment figure on the attachment behavior of young children. It would seem that a potential source of information in this area might be the literature on child abuse and neglect, through which we sifted thoroughly. Most of the evidence we located was embedded in the context of single case reports or anecdotes. We found some claims that abused children showed heightened attachments, and some claims for diminished attachments, but the nature of the evidence was such that no defensible conclusion can be drawn at this stage. Perhaps the most commonly reported response to abuse is the emergence of a heightened awareness of the behavior of adults, and a marked ability to foresee their likely reactions (Martin, 1976). This permits children to avoid rather than escape abusive behavior.

#### Summary

We have uncovered no evidence that the maltreatment of infants materially interferes with the formation or persistence of social bonds in the species for which data exist. Some procedures are clearly effective in changing response rates, although these temporary effects themselves are not always consistent, since in different species aversive conditions either reduce or enhance approach tendencies temporarily. In addition, it would be erroneous to conclude that these temporary (or relative) changes in filial responsiveness all reveal alterations in the social bond, since other tests often show that the level of social attachment is unaffected.

## Fitting theory with maltreatment data

**Maltreatment effects and ethological theory.** Bowlby's theory predicted that the attached infant would be strongly drawn to the attachment figure under conditions of stress, even if the attachment figure itself was the source of the stress. Some of the data we reviewed are in line with this prediction. For instance, Harlow and Harlow (1971) claim that evil mother treatments enhanced infantile attachment in monkeys, Fisher's (1955) indulged-punished pups were as much as twice as socially responsive as were the indulged group, and Salzen (1970) reported that chicks showed an increase in their attraction to an imprinting target after the target struck them sharply on the head.

On the other hand, certain of the ethologically oriented writers have suggested, more or less by implication, that maltreatment should in some way limit or inhibit the formation of the social bond. Although he is not specific, Bowlby (1969, p. 273) mentioned that the behavioral sequences involved in infantile attachment could be diminished by the feedback of certain results, and we gather that the experience of electric shock, painful temperature, or rough handling are the sorts of results Bowlby had in mind on this point. Similarly, Lamb (1976, 1978) argued that attachment would not occur unless the social object behaved "appropriately" toward the infant. Neither of these positions is borne out by the maltreatment literature.

Indeed, attachments were formed to objects that maltreated the infants from the outset. For example, the airblast schedule employed by Rosenblum and Harlow (1963) was in effect on the day the monkeys were introduced to their surrogates, yet the social bonding of these infants was not impaired. Further, in Fisher's (1955) punishment condition, puppies were treated in an obviously abusive manner from the beginning of contact with the handler. Even so, these animals did not completely avoid interaction with that handler, and three of the six dogs in this condition received a nonzero orientation score in the tests that followed the prolonged period of abuse. Finally, precocial birds can be quite socially responsive, even when they first encounter the potential social object under conditions of painful shock (Kovach & Hess, 1963; Barrett, 1972) or crippling cold (Fischer, 1970). Since Bowlby and Ainsworth propose that mere extended exposure is sufficient for attachments to develop, their predictions are not inconsistent with these findings.

However, it should be pointed out that the ethological theorists feel that the behavioral systems (or capacities) of the infant have been shaped to function in an expected or probable environmental niche. As Ainsworth (1969, p. 1000) put it, "the function of a system is the one that gave it species-survival advantage in the 'environment of evolutionary adaptedness'—the original environment in which the species first emerged . . . . Genetic programming continues to bias the infant to behave in ways adapted to the original environment of evolutionary adaptedness. . . ." To take an extreme example, if an infant is born in an atmosphere that is devoid of oxygen, some of its biological systems will be unable to function because those systems evolved in an oxygen-laden environment. Unfortunately, it is much less obvious how to make somewhat finer distinctions in this area, and where to draw the line between environments that are expectable or suitable, marginally suitable, or completely unsuitable. In terms of the behavior of social objects, can we possibly view abuse or maltreatment as constituting part of an ordinarily expectable environment? These conditions hardly seem conducive to the survival of the offspring, yet infants do become attached to objects that severely maltreat them. Ainsworth (1969, p. 1008) might view this sort of bond as an anomaly, but if it is an anomaly the deviation is represented in the behavior of the attachment figure, not in the infant.

In sum, ethological theory correctly predicts the persistence of attachment behavior (that is, the influence of the set-goal) under conditions of parental abuse commencing after specific social

bonds have formed. There is also evidence that bonds can form under conditions of maltreatment. In accounting for the development of social bonds, ethological theory generally places an emphasis on the appropriateness of the attachment object's behavior toward the infant. The data show that bonds form despite unresponsiveness or inappropriate responsiveness.

**Maltreatment effects and the classical conditioning model.** The first prediction concerning the effects of maltreatment that was derived from the classical conditioning model suggested that attachments form (or imprinting takes place) because certain stimulus configurations reduce the subject's arousal. However, in several studies (Fisher, 1955; Rajecki et al., in preparation; Sackett et al., 1967; Rosenblum & Harlow, 1963) the social object was itself the agent of maltreatment, and it seems reasonable to assume that such activity produced arousal in the subject, rather than causing a reduction in arousal. Nevertheless, under these circumstances the infants reacted (eventually) with prosocial responses to the abusive figure.

The second prediction from this model was that aversive stimulation during bonding episodes should enhance attachment. However, while certain procedures produce *either* heightened or reduced responsiveness to social objects under aversive conditions, several studies (Barrett, 1972; Ratner, 1976) show that bonds form at some absolute level. It was pointed out that the apparent enhancement or inhibition of social behavior under various aversive contingencies was likely to be due to transient tendencies or situational constraints and not to the strength of the infant's attachment.

To summarize, the classical conditioning interpretation does not receive strong support from the maltreatment literature. Especially damaging are the data from studies in which the social object is the source of the abuse, because it is difficult to reconcile these findings with the idea that a reduction in arousal is the key component in the bonding process.

**Maltreatment effects and the instrumental/operant learning theory.** Since Gewirtz bases his interpretation of attachment on "positive stimulus control," this implies that the maltreatment of the infant by the social object (or maltreatment in association with the attachment object) at any stage in the bonding process would lead to diminished attraction or even active avoidance of the object. This is not the case in practice. There is no evidence that the basic social bond is blocked or disrupted by maltreatment. While the punishment contingency is in effect, the infant may temporarily modify its filial responses in order to avoid or escape the noxious stimulation. When the contingency is removed, however, the prosocial behavior is reinstated. Because the development and perseveration of the social bond is essentially *independent* of negative reinforcement or punishment, it seems, therefore, unlikely that the formation of such bonds is *dependent* on positive reinforcement.

In sum, there are considerable difficulties in reconciling maltreatment effects with the tenets of a positive reinforcement analysis of attachment. The maltreatment literature offers no support for the reinforcement idea and represents a significant obstacle in the way of accepting such a theory.

**Maltreatment effects and Cairns' contiguity analysis.** The contiguity analysis is the only theory that speaks directly to the issue of the maltreatment of the infant during the bonding process, and, interestingly enough, it suggests that such punishment should not directly influence the formation of attachment. Insofar as the existing data permit a test of this notion, the prediction receives some confirmation. A number of studies show that bonds form regardless of maltreatment, and therefore the first of Cairns's hypotheses gains support.

**Maltreatment effects and the opponent-process model.** The opponent-process model seems to be unequal to the task of eluci-



dating the effects of maltreatment by attachment figures. First, Hoffman and Solomon (1974) assume that the presence of an appropriate object causes a positively toned A-state and thus that the object's absence results in a negatively toned B-state. The data show, however, that social bonds develop under conditions that must have produced a negative A-state in the infant. Second, the theory appears incompatible with much of the evidence concerning the effects of maltreatment after bonding has taken place. If A-states are additive, then approach responses should be reduced during maltreatment, whereas many of the data show enhanced proximity-seeking. If A-states are not additive, then it is unclear how one would predict any consistent effect of maltreatment.

## Secure base effects

### Secure base effects in birds

A number of studies indicate that the presence of siblings or the birds' mother increases the likelihood that precocial avian subjects will emit exploratory or aggressive pecks (Collias & Collias, 1956; Hogan & Abel, 1971). However, in this section we will restrict the discussion to studies in which the birds' companions were artificial (inanimate, even if animated) imprinting targets. This assures that the secure base effects identified can be attributed to the psychological state of the subject and not to the directive or signalling properties of the test mate.

Stettner and Tilds (1966) found that ducklings tested in an open field in the absence of their attachment object (a green cube) would maximize the distance between themselves and a fear-provoking stimulus (a Raggedy Ann doll). On the other hand, when the imprinting object was placed between the fear stimulus and the bird, the birds approached the cube despite the fact that to do so meant that they had to approach the doll as well. The presence of the inanimate companion to which the subject is attached is also known to enhance hatchlings' pecking. Wilson and Rajecki (1974, Experiment 2) reared individual chicks with red styrofoam objects and then placed their subjects in a situation that contained a novel object (a restrained chick) and either a familiar (red) or unfamiliar (green) imprinting object. The subjects were more likely to peck at the novel chick in the presence of the familiar object than in the presence of the one that was unfamiliar.

A graphic illustration of the impact of an attachment object on the subject's willingness to deal with the environment comes from Hoffman and Boskoff (1972). These researchers reared half their sample of ducklings in pairs ("the socialized birds"). The remaining ducklings ("the imprinted birds") were reared individually, but were given several daily exposures to a moving imprinting object (a foam rubber block). In tests, Hoffman and Boskoff placed one socialized and one imprinted bird together in a small arena within which the foam rubber object could be displayed. For the imprinted birds, the presence or absence of the object had a great impact on the degree of aggression toward the socialized test mate. The imprinted birds appeared to dominate their socialized test mates completely, (via social pecking), but only when the imprinting target was present. In the target's absence, the imprinted birds fled from the other duckling.

In a related study, Rajecki, et al. (1978b) reared individual chicks with either a red or green styrofoam object and then assessed pairs of animals for conspecific aggression (as revealed by head pecking) in the presence of a familiar or unfamiliar object. Aggression was observed in tests as early as twenty-four to forty-eight hours after hatching, and success in these encounters was influenced by the presence of the inanimate social object. In 68 percent of the social pecking tests the winner of the encounter (giving more pecks than it received) was the chick for which the object was familiar.

As suggested earlier, effects such as these are interesting because the birds' behavior is due to their own psychological state, and not to cues or signals from their companions. It is not clear, however, whether this behavior is promoted by the presence of the attachment object, or whether the removal of the object disrupts ongoing behavior. A corollary question has to do with the uniqueness of the "attachment" object. Young birds are drawn to and seem to be contented by social objects, but they are also drawn to nonsocial manipulanda that distribute positive reinforcement. Could it be the case, then, that any object that was attractive to the bird (for any reason) might influence the bird's behavior in the way it is influenced by "attachment" objects?

Apparently, the answer to this question is "no." Marley and Morse (1966, Figure 7) removed socially reared chicks from their communal setting and tested them individually in a chamber in which the birds had previously been trained to peck at a key for food. Early in the test sessions the chicks pecked at the key avidly and gave no distress calls. After an hour or so, satiation occurred, and the chicks stopped pecking at the key. The birds then began to emit a high rate of distress vocalizations, a reaction that occurs when chicks are separated from companions (Bermant, 1963; Kaufman & Hinde, 1961; Rajecki et al., 1977). It would seem, therefore, that the operant device employed by Marley and Morse (1966) only influenced the behavior of their chicks as long as it had a specific utility, and that it did not provide the kind of nonspecific security (i.e., governing approach responses, pecking at novelty, and aggressive pecking) that attachment objects do. (There are, of course, limits to the benefits a chick can derive from the presence of an inanimate attachment object. Thompson and O'Kieffe [1962] removed all food and water from the cages of sixty-hour-old chicks. Some of these birds had previously been imprinted to a cloth target, and had this target as a cohabitant in the rearing cage. Other chicks had been reared in isolation throughout. Despite this vast difference in early social experience, no differences between groups were found in terms of time to starvation or the amount of weight loss at death.)

### Secure base effects in dogs

Several attempts have been made to determine whether the presence of a conspecific alleviates distress reactions induced by separation from mother or littermates, placement in an unfamiliar situation, or restraint. Some published reports confirm that the presence of another dog dramatically reduces behavioral distress (Fredericson, 1952; Ross et al., 1960; Scott & Marston, 1950). On the other hand, the presence of a companion does not markedly affect the degree of response to a fear-provoking sound (Davis et al., 1977), and animals tested in pairs were slower to approach an unfamiliar object than were animals tested singly (Davis et al., 1977). Moreover, results of another recent study raise additional concerns about the existence of the secure base effect in dogs. Pettijohn et al., (1977) found that distress at separation (from mother and littermates) was greatly reduced when the puppy was accompanied by a human being, somewhat less reduced when the companion was an unfamiliar bitch, slightly reduced when a soft toy was present, and essentially unaffected when food was provided. The canine companion was less effective than a human one, reasoned Pettijohn et al., because the older dogs explored the test room and ignored the puppies, whereas human beings initiated interaction or accepted the puppies' advances.

These findings suggest that security is assured, not simply by the presence of an individual, but by its behavior. This would not represent an example of the secure base effect. Indeed, we find ourselves questioning whether a true secure base effect has been demonstrated in the canine research. Unfortunately, we are unaware of any studies in which the distress-reducing properties

of familiar and unfamiliar companions are systematically compared. If many companions in the puppy's world are effective in providing security, then in using these data one cannot claim to be tapping the effects of attachment to a specific individual.

### Secure base effects in monkeys

The literature contains several convincing anecdotes concerning the capacity of an attachment object to serve as a secure base for infant monkeys (Harlow & Harlow, 1965, 1972; Mason, 1970), and the data in this area agree with the informal descriptions, at least with respect to the influence of surrogates on the emotionality of infant macaques (see Jay's [1963, p. 292] description of early exploration in young langurs). Candland and Mason (1968) placed young monkeys in an unfamiliar room either with or without a towel that served as the infant's social object, and measured the heart rate of the subjects. Heart rate was lower in the presence of the towel than in its absence, and the monkeys habituated to the strange place much more quickly when they had something to cling to. In a related physiological investigation by Hill and McCormack (cited in Mason, 1970, Figure 9), the cortisol level of young monkeys in a strange environment was lower when the animals had access to a surrogate, compared to when the surrogate was not available. Mason interprets this finding to mean that the presence of the surrogate had a calming influence on the monkeys.

There is, of course, evidence at the behavioral level that the presence of an adequate attachment object reduces the emotionality of infant monkeys. Harlow and Zimmerman (1959) report that the presence of a familiar cloth surrogate reduced the emotional responses (vocalizations, self-directed behavior) of monkeys in a test situation, and that this effect obtained whether or not the animals had fed from that surrogate. Even more interesting, perhaps, was the finding that monkeys who had been reared with and fed by a wire mother exclusively did not show attachment to that surrogate and did not use it as a secure base (see Harlow & Zimmerman, 1959, Figure 14). For the rhesus, at least, a surrogate that merely reduces the hunger drive need not assume any social significance in the monkey's life.

Mason and Berkson (1975) also conducted behavioral tests of the secure base effect for surrogates and clearly replicated the finding that affect is greatly influenced by the presence or absence of the attachment object. When monkeys were tested alone in a relatively unfamiliar cage they showed reliably more distress vocalizations, self-clasping, defecation, and urination than when tested with the surrogate present. Furthermore, when novel objects (e.g., doll, block of wood, rope) were introduced to the home or test cage, the absence of the attachment object was again associated with higher levels of negative affect. Here, however, there were increases in locomotion, manipulation of the environment, and contact with the novel objects in the surrogate's absence (compared to its presence), at least in the home cage. (When tested in the novel cage the monkeys displayed less locomotion and environmental manipulation than when tested at home.) In sum, Mason and Berkson (1975) provide sound evidence that emotional responses induced by changes in the environment are affected by the attachment object's presence and by somewhat equivocal findings concerning secure base effects on exploration.

### Secure base effects in children

In the course of a naturalistic, longitudinal study, Ainsworth (1963, 1964, 1967) found that with the emergence of locomotor capacity, infants would move away from their mothers on exploratory forays, returning to them at intervals as if for reassurance (cf. Mahler's, 1968, idea of "emotional refueling"). Subsequent naturalistic studies showed that the frequency and

duration of such forays increased with age up to thirty six months (Rheingold & Eckerman, 1970; Anderson, 1972; Ley & Koepke, 1975), and that the periodic returns were not elicited by maternal summonses (Ley & Koepke, 1975). Anderson (1972) and Ley and Koepke (1975) reported that younger infants were more likely than older ones to require retrieval. Similarly, the younger infants in Anderson's study stood immobile when their mothers stood up to leave, whereas the older children approached their mothers rapidly.

Several laboratory studies have also demonstrated that the presence of the mother facilitates exploration. Cox and Campbell (1968) found that both thirteen- to fifteen- and twenty- to thirty-seven-month-olds spoke, played, and moved about more in the mother's presence than in her absence, although the effect was stronger among those in the younger age group. A diminution in the amount of exploratory play when an attachment figure leaves the scene has been reported for several samples of twelve- to thirty six-month-olds (Ainsworth & Wittig, 1969, Feldman & Ingham, 1975; Maccoby & Feldman, 1972).

Passman (1974, 1976; Passman & Weisberg, 1975) has conducted a series of studies in which security blankets were used as secure bases. Passman and Weisberg (1975) found that "blanket-attached" toddlers explored as much in the presence of their blankets as in the presence of their mothers, whereas "blanket-nonattached" toddlers gained little security from the presence of a blanket. In a later study, Passman (1977) showed that the presence of either a mother or a security blanket facilitated achievement on a discrimination learning task. Finally, Passman and Erck (1977) showed that the physical presence of the attachment object was not necessary: pictures of the mothers of preschoolers were sufficient to provide security. In short, this series of studies showed that secure base effects could be demonstrated to occur with inanimate objects as well as with persons serving as attachment objects.

Several studies have shown that the presence of an attachment figure facilitates positive social interaction with strangers. Most impressive are the findings that infants evince more stranger distress when seated a few feet away from their mothers than when held by them (Scarr & Salapatek, 1970; Morgan & Ricciuti, 1969; Sroufe et al., 1974). Rheingold and Eckerman (1973) report that infants engage more readily in interaction with strangers when their mothers are with them than when they are absent, while Kotelchuck (1976) found that the presence of mother or father served to reduce the extent of negative stranger reaction.

Most of the research on secure base effects in human beings has been concerned with average tendencies. Ainsworth, however, has examined individual differences in the ability of infants to use their mothers as sources of security (Ainsworth & Wittig, 1969; Ainsworth et al., 1971, 1974). The children's behavior reflected varying degrees of adaptive functioning. Some infants explored when their mothers were present, responded with distress and decreased exploration upon their departure, and with warm greetings followed by renewed exploration upon their return. As defined by the ethological attachment theorists, such behavior appeared adaptive. Contrarily, other infants were unfazed by the departure of their mothers, and either avoided them, or treated them angrily or ambivalently when they returned. These responses appeared maladaptive. The differing patterns of behavior seemed to be consequences of earlier patterns of mother-infant interaction (Ainsworth et al., 1974). Infants who behaved adaptively had mothers who responded appropriately and consistently to their signals, whereas those whose behavior was judged maladaptive had mothers who had earlier been significantly less sensitive.

### Summary

The evidence for secure base effects is poorest for dogs, about whom relevant data are apparently lacking. For the remaining

species, the evidence is quite compelling. Birds, monkeys, and children do benefit from the presence of an attachment object that promotes exploration and other adaptive responses. Two findings of special interest emerge from this literature. First, not all objects or agents in the infant's environment function as sources of social security. Monkeys do not gain support from wire surrogates that feed them, and the behavior of chicks is not broadly affected by the presence of a food-dispensing manipulandum. Therefore, not all dispensers of reinforcers function as secure bases. Second, inanimate objects serve as secure bases for birds (styrofoam objects), monkeys (cloth surrogates), and children (blankets). This clearly implies that the security derives from the psychological state of the infant, and not from cues emitted by the attachment object.

### Fitting theory with secure base data

**Secure base effects and ethological theory.** This theory proposes that the presence of an attachment figure will facilitate exploration of the environment and reduce the stress of contact with other, unfamiliar entities. Among birds, dogs, monkeys, and human beings, the presence of a companion indeed provides security for young infants. We do know that for birds, monkeys, and human beings the identity of the individual is important, whereas there is not yet acceptable evidence that for dogs the companion has to be a specific familiar individual. Clearly, the bulk of the comparative data are in line with the theory's prediction concerning the influence of the set-goal on the organization of behavior. Further, there is some evidence that the sensitivity and appropriateness of the parents' earlier behavior can have a measurable impact on their capacity to serve as a secure base, a finding also consistent with the ethological theory.

The fact that inanimate objects (e.g., styrofoam objects, cloth surrogates, and blankets) can serve as secure bases is not entirely compatible with the theory's predictions. The implication is that such inanimate materials can function in this way *because* they are the objects of the infants' attachment. If this is so, one has to account not for the security, but for the attachment itself. According to the theory, proper or secure attachments form to individuals who emit appropriate responses to infant signals. Why, then, do infants become attached to inanimate objects that cannot respond at all? It might be argued, alternately, that children's attachments to their blankets (for example) are attributable to the "contact comfort" properties of the blankets. But all infants have access to blanket, towels, and other comfortable and portable materials, yet apparently not all gain a sense of security from this class of items.

The issue of the "behavior" of the attachment object illustrates a deficiency of the ethological theory that was suggested in the section on maltreatment effects, although the criticisms derive from very different classes of evidence. In that section we concluded that strong attachments may form despite the style of interaction between infant and adult. To this we can add another statement: Attachment figures can serve as secure bases regardless of the previous style of interaction between the individuals involved. Neither of these conclusions seems to follow directly from the notion that infants are predisposed to be influenced by the expectable features of the social environment, unless the theory can include inanimate objects in such an environment.

**Secure base effects and the classical conditioning model.** Hoffman's position is that the presence of an attachment object can mediate an imprinted bird's aggression toward a novel object by reducing the subject's level of arousal. This leads to the prediction (elaborated above) that older birds will be more aggressive than younger birds in the presence of a familiar object. However, this age effect does not always obtain. In Rajecki et al. (1978a) chicks evidenced secure-base effects on aggression at

twenty-four to forty-eight hours of age. This is inconsistent with the prediction. In the first place, these birds were generally younger than any of those in the Hoffman et al. (1974) study, and, in the second place, a reexamination of our raw data indicated that, if anything, the *younger* birds behaved more aggressively in the presence of a familiar object than did those who were somewhat older.

Of course, age is not the only factor to be taken into account when comparing the data of Hoffman et al. (1974) with Rajecki et al. (1978a), but the discrepancy between the two results remains problematic for the arousal-reduction analysis. Further, it is not clear how the arousal-reduction notion would account for the influence of the attachment object on the infant's nonaggressive responses to the nonsocial environment. In fact, there seems to be something underlying certain secure base effects that is actually motivating the infant to engage in exploratory forays, and not merely allowing it to do so via the disinhibition of fear-inhibited responses. For example, Rheingold and Eckerman (1970) reported that human infants would explore areas out of sight of the mother, and that the babies alternated between approaching and departing from the mother. However, approaches to the social object were not necessarily motivated by fear, since "the return was often accompanied by facial and vocal expressions of pleasure and not by signs of fear or of relief from fear" (Rheingold & Eckerman, 1970, p. 82).

With regard to the effect of an imprinting object on aggression, Hoffman and Ratner (1973) claim "that the imprinting stimulus modulates a duckling's aggression and filial behavior toward a novel stimulus by lowering its fear of the stimulus" (p. 539). If so, it is quite unclear why, according to Hoffman's conditioning principles, a familiar object should exert this form of influence, since nothing in the five premises noted above suggests such an outcome. It is clearly Hoffman's contention that ducklings can discriminate between the familiar and novel items, and that reactions to the former somehow inhibit, reduce, or override reactions to the latter. However, since the fear reaction in an older bird to a novel stimulus is conceived to be as innate or reflexively determined ("maturationally based," Hoffman & Ratner, 1973, p. 531) as the innate disposition to respond filially to the younger bird, it seems, therefore, that some sort of corollary or combination rule is required to resolve the conflict between the two automatic reactions to the two kinds of stimuli. Hoffman and Ratner (1973) state in their fifth premise that "the behavior an immature precocial bird displays toward a *given* imprinting stimulus reflects a resolution of the competing tendencies aroused by the stimulus to respond filially and fearfully" (p. 531, *italics added*). Nevertheless, it seems to us that by the time an imprinted duckling is five days old (as in Hoffman et al., 1974) it would, in the presence of *both* objects, respond completely filially to the one that was familiar, and completely fearfully to the one that was novel. Paradoxically, we find a similar suggestion in the fourth Hoffman-Ratner premise: "We also assume that since the fear response is predicated on the detection of novelty, its strength will in part depend upon the nature of the stimulation previously experienced and on the perceptual incongruities that a given novel stimulus engenders" (p. 531). Would it not be the case, we wonder, that a new object would seem most novel (and therefore, fear-provoking) when in direct contrast with one that was familiar?

To summarize, the classical conditioning model does not offer a compelling account for secure base effects. While one can accept the idea that arousal-reduction (as mediated by the social object) influences some response patterns, it does not seem that this explanatory device is widely applicable.

**Secure base effects and the instrumental/operant learning theory.** If one is willing to concede that the social object is potentially a source of positive primary or secondary reinforcers, there is still no obvious reason why the reinforcement theory



would predict the secure base effect. Even if there was a potential mechanism (some sort of special generalization process, for example), the data show that not all reinforcing agents come to function as secure bases. For instance, lactating wire mother surrogates (monkeys) and keypecking devices (chicks) are useless once they have lost (through satiation) their highly specific usefulness. Meanwhile, objects that do not obviously dispense reinforcement (styrofoam objects for chicks and ducklings and photographs for certain children) can serve the secure base function.

In sum, although secure base effects comprise an integral and interesting aspect of the phenomenon of attachment, the instrumental/operant learning theory does not help us to understand them.

## Separation effects

### Separation effects in birds

Studies of short-term social separation in precocial birds are fairly abundant and most show that chicks and ducklings are very likely to emit distress vocalizations when separated from companions. A recent addition to this literature is worth noting. Gaioni, et al. (1977) reared ducklings in groups of several subjects and then temporarily removed various numbers of birds from each group. The general finding was that the fewer the birds remaining in a group, the more likely they were to give distress calls. Interestingly, the addition of new ducklings to established groups did not promote distress calling. Thus, short-term separation effects are not due merely to the sheer number of ducklings in the situation; there is something special about the loss of companions.

With respect to the effects of prolonged separation, Rajecki et al. (1977) reared chicks in pairs and then separated cagemates at two weeks of age. A minority of the separated birds (five of the sample of twenty) showed little reaction to the loss of a companion, and continued to behave at pre-separation levels. However, a majority of the sample showed one of two types of adverse reaction. Nine of the birds spent most of the first five minutes following separation in a passive or immobile state. The degree of inactivity of this group remained high over the first day of separation, and continued above baseline level for the remaining seven days of the study. This was impressive since the birds were seldom inactive during observations made in the pre-separation period. The remaining six chicks reacted to the initial separation by spending the bulk of their time emitting distress vocalizations. After several hours of segregation the distress calling of these subjects abated somewhat, and the animals fell into the inactive state. Rajecki et al. (1977, p. 150) pointed out that the response profile for this last group resembled the anaclitic depression syndrome observed in primates (see below). For this final group, levels of distress vocalization and inactivity continued above baseline levels for the remainder of the research.

Later, Rajecki et al. (1978b) investigated the impact of multiple social separations in chicks. Cagemates were separated and reunited (for one- to 3-day periods) fifteen times during the first seven weeks after hatching. Distress vocalizations and measures of inactivity showed that the birds were sensitive to every separation, and a social pecking measure demonstrated sensitivity to every reunion. These chicks did not show the developmental arrest evident in young monkeys subjected to multiple separations (see Suomi et al., 1970) although Rajecki et al. (1978b) did detect a cumulative change in one facet of social behavior. Specifically, there was a strong and stable tendency for nonseparated chicks to match the food peck rates of their companions. Matching was initially high for the separates as well, but by the end of the series of repetitive separations the separates failed to match one another.

### Separation effects in dogs

All else being equal, puppies reliably and rapidly evince distress at separation (Pettijohn et al., 1977). Elliot and Scott (1961) found that the peak rate of distress within a ten-minute separation period was reached within three to four minutes. Further, Cairns and Werboff (1967) noted that in one-month-old dogs, an asymptotic distress rate occurred after as little as twenty-four hours of prior cohabitation with a nonconspecific (a rabbit). There has also been some investigation of ways of alleviating distress reactions; some of these were discussed in the secure base section. At this point we will only note that familiar housing conditions, the presence of familiar objects or individuals (Elliot & Scott, 1961), or confinement to a sound-proof room (Scott & DeGhett, 1972) reduce the distress response, whereas physical restraint increases the degree of distress evinced (Fredericson, 1952; Ross et al., 1960). Elliot and Scott (1961) showed that with age controlled, the greater the pup's experience with separation, the less distress it displayed. Animals separated for the first time at twelve weeks of age were maximally distressed, and their rate of yelping increased linearly across a ten-minute trial. Several other studies, using a variety of separation-reunion schedules, have confirmed that degree of distress is inversely related to prior separation experience (Ross et al., 1960; DeGhett et al., 1970; Stewart et al., 1970).

One feature of the literature on separation effects in young dogs deserves special attention. While most observers of separated infants (of whatever species) would probably agree that the infant undergoes a noxious psychological experience or negative emotional state, the precise quality of this affect is usually unspecified. In the case of puppies, however, Scott (Scott & Bronson, 1964; Davis et al., 1977) contends that the reaction to separation is unique in that it can be differentiated from other emotional or motivational states. Scott's claim is based on the observed interactions among emotional states. For example, the reaction to separation and the reaction to a novel situation are not viewed as the same thing, because these two reactions have additive effects on vocalizations (Scott & Bronson, 1964, Table 3). Similarly, distress at separation and the discomfort of hunger are not the same thing, because separation has a suppressive effect on vocalizations caused by food deprivation (Scott & Bronson, 1964, Table 4). Further, Davis et al. (1977) were able to distinguish between separation-induced *distress* and *fear* that was induced by an auditory stimulus. Puppies tested alone vocalized more than when they were tested together, but the sound stimulus did not influence this rate. On the other hand, the sound resulted in changes in the animals' tail carriage (they tucked their tails), but this response was unaffected by social condition.

Unfortunately, we are unaware of any systematic researches concerned with long-term separation effects in pups. There is one impressive observation in the literature, however. Scott et al. (1973) reported that the lengthy separation (twenty-two out of twenty-four hours per day) of five- to seven-week-old puppies resulted in especially severe emotional upset, and may have contributed to the death of one of them.

### Separation effects in monkeys

Since several recent and comprehensive reviews on this topic are available (Hinde & McGinnis, 1977; Mineka & Suomi, in press), we will discuss the data selectively, reiterating some of the highlights of this research. One reliable finding is that infant monkeys that have been segregated from their mothers initially protest (emitting distress vocalizations, coupled with agitated locomotion) and then fall into a kind of despair or depression (marked inactivity, depressive-like expressions or postures). A number of writers have compared this reaction to the anaclitic depression syndrome observed by Bowlby (1960) and Spitz (1946) in children (Kaufman & Rosenblum, 1967a, 1967b; McKinney & Bunney, 1969; Harlow & Suomi, 1974).

This pattern can be influenced by several factors. Infants that remain in the home cage (while their mother is removed) show relatively short protest periods, and lapse quickly into the inactive state. On the other hand, infants that are placed in an unfamiliar environment during separation show a prolonged protest period. The pattern of social organization prior to separation also seems to affect the separation response. This is best illustrated by studies of the differential reactions seen in pigtail and bonnet macaque infants. Rosenblum (1971a; & Kaufman, 1968) reports that whereas a pigtail mother will not permit other adults to interact with her infant, a bonnet mother is likely to let other bonnets explore, handle, and groom the neonate. Further, the pigtail mother restricts the departures (from herself) of her developing offspring, while the bonnet engages in only a fourth as much departure restraint. One consequence of these different mothering styles is that the bonnet infant easily weathers its mother's absence by directing its social responses to other adults in the colony, whereas the pigtail infant is distraught. Rosenblum (1971a) has also found that polymatric rearing ameliorates separation distress in the squirrel monkey.

The long-term effects of separation procedures may be even more impressive than the short-term effects. Suomi et al. (1970), for example, separated and reunited small groups of monkeys twenty times over a period of several months and found the protest-depression pattern within every separation and reunion episode, showing that in monkeys short-term reactions do not habituate. Protest diminished over episodes and was supplanted by despair (see also Suomi et al., in preparation). More importantly, perhaps, these multiple separations exercised a major influence on the development of the individuals. In control subjects, infant-infant clinging diminished from Day 30 to Day 270 of development, but repeatedly separated 270-day-old monkeys continued to cling (upon reunion) at Day 90 levels. Moreover, although social play is predominant in interactions among normal nine-month-old monkeys, this form of behavior did not emerge in the multiply separated subjects. In short, the multiple separations seemed to produce infantilization through maturational arrest. (For evidence of other long-term debilitating effects see Suomi & Harlow, 1975).

### Separation effects in children

Spitz (1946) and Bowlby (1951) offered vivid descriptions of the pathetic condition of institutionalized children. Both writers pointed to an affectless demeanor, a lack of enthusiasm for interaction with people or objects, and a marked introverted withdrawal. Institutionalized infants seemed especially susceptible to disease and infection, largely, claimed Spitz, because they had nothing to live for.

Robertson and Bowlby (1952) studied children who had been confined to isolation sanatoria and were therefore separated from their parents. These authors delineated three stages in the response to separation. The initial stage was termed protest: children in this phase were tearful and resisted attempts by substitute caretakers to soothe them. Outbursts of anger were common if the separation was an extended one. Next, the child entered a phase called despair, which was remarkable for the absence of overt signs of protest. Robertson and Bowlby mention that the hospital staff viewed this change in condition as evidence that the child had made a "partial adjustment." However, the authors saw this state as one in which the child was actually apathetic, withdrawn, and in deep mourning. Finally, there came a stage that Robertson and Bowlby named denial/detachment. The withdrawn behavior characteristic of despair disappeared, and the child began to interact normally with peers and caretakers.

Later researchers sought to extend the generalizability of the separation phenomenon by examining otherwise healthy children. For example, Heinicke and Westheimer (1965) observed children who were placed in residential nurseries while

the youngster's mothers were hospitalized for childbirth, and reported a pattern of responses remarkably similar to those described by Robertson and Bowlby (1952).

The response to separation differs depending upon the age of the child concerned. Schaffer and Callender (1959) reported that infants of twenty-eight weeks of age and younger were not affected by hospitalization, whereas those of twenty-nine weeks and older regularly were affected. This finding is consistent with the report by Yarrow (1967) that infants of less than six months of age could be transferred from foster to adoptive homes without difficulty, whereas older infants passed through a period of separation protest before adjusting to the new family context. Most of the children studied by Robertson and Bowlby (1952) and Heinicke and Westheimer (1965) were eighteen months of age and older when observed.

The intensity of the response to the separation is clearly not invariant. Robertson and Robertson (1971) describe a series of case studies in which they took individual children into their own home during separations from their families in order to determine whether emotional preparation and the presence of a full-time substitute caretaker could minimize the separation response. Their results indicated that the intensity of the protest response was greatly diminished in these circumstances, and that the passage through the other phases of response (despair, detachment) could be prevented.

Experimental research on human separation effects has focused on the first (protest) phase of the response, and the results of several studies have been clear cut. Separation from an attachment figure, be it mother or father, elicits protest more readily than does separation from an unfamiliar person (Cohen & Campos, 1974; Fleener & Cairns, 1970; Kotelchuck, 1973; Stayton et al., 1973; Schaffer & Emerson, 1964). Several factors influence the occurrence of separation protest. Differential protest, for example, appears at home late in the second quarter-year of life (Stayton et al., 1973), whereas protest to laboratory separations is not differential until twelve months of age (Kotelchuck, 1972; Fleener & Cairns, 1970). Furthermore, Kagan and his colleagues point to the effects of familiar surroundings on the degree of response to separation. Ross et al. (1975) reported more separation protest in the laboratory than in the home, and Littenburg et al. (1971) found that there was more infantile distress when the mother left through a novel exit rather than the usual door of a familiar room.

### Summary

For all species, there is good evidence that the forced separation of the infant from its social object can (depending on the qualifications noted) be a traumatic experience. Normal or ongoing behavior is profoundly disrupted, and with severe or repeated separations, the result can be biological, psychological, and social retardation. Some of these separation effects are immediate and extreme, but the sheer *quantity* of the reaction is less impressive to us than the *quality* of the reaction. Human and monkey infants can be reduced to a state of despair by separation, and in the puppy the affective state that separation engenders can be discerned from affective states caused by other kinds of deprivation or stimulation.

### Fitting theory with separation data

**Separation effects and ethological theory.** The fact that the separation data are partially consistent with the predictions of ethological theory is not surprising. As noted earlier, clinical experience with the effects of human separation played a key role in the formulation of the theory.

Theorists such as Bowlby (1973) argue that the response to separation depends on both the quality of the relationship before separation and the characteristics of the substitute attachment

figure (if any). It is noteworthy, however, that the process whereby an original bond is broken (by separation) so that new attachments may form need be neither lengthy nor particularly traumatic. Among chicks, significant shifts in preferences can occur as little as five hours after the original object is replaced (Einsiedel, 1975). Furthermore, Mason and Kenney (1974) report that young monkeys from a variety of rearing conditions (surrogate, peer, biological mother) did not succumb to the prolonged anacletic depression syndrome when separated from original social objects. Instead, all the monkeys in this study quickly began to form strong and apparently satisfying social bonds with their new cohabitants, a set of mongrel dogs. Mason and Kenney observed that seven of the eight monkeys in their study clung to their canine cohabitant within four hours of introduction, and that the remaining animal showed this response after about thirteen hours. (See also the work of Robertson & Robertson, 1971, on variations in children's responses to separation.) These rapid shifts in attachment responses imply that the nature of the link between profound separation effects and the prior social bond is not as clear-cut as ethological theorists propose. Further, this sort of finding brings into question just what "amount" of contact is required before specific bonds form, or whether "amount" of contact is indeed the key factor in bonding.

**Separation effects and the classical conditioning model.** As noted, the classical conditioning model says little concerning the effect of involuntary separation. Most infants appear to be quite aroused by the loss of an attachment object, and this is consistent with Hoffman's conceptualization. However, as separation effects seem to involve far more than a simple increase in arousal (i.e., the depressive phase), the model can speak to only part of the phenomenon.

**Separation effects and the instrumental/operant learning theory.** Gewirtz accounts for the intensity and duration of reactions to involuntary social separation by appealing to the presumed reinforcement history of such responses in the organism's past. According to this view, the infant learns that its protestations are associated with (or instrumental in) the return of the desired object, and therefore on subsequent separations protest will be used in an attempt to recover the attachment figure. While this analysis is plausible, it cannot account for what is known about separation protest. As noted in the discussion of Scott's (1971) learning theory of social motivation, multiple prior separations are not necessary precursors of intense reactions to a given separation. Furthermore, Gewirtz's position is contradicted by the evidence that experience with separation can (for whatever reason) produce a reliable decrease in the magnitude of separation protest in chicks (Rajecki et al., 1978b), puppies (Elliot & Scott, 1961), and young monkeys (Suomi et al., 1970). (What may increase over repeated separations is despair, but this represents, in our view, a qualitatively different form of response than separation protest.)

In view of these problems, Gewirtz has provided an unsatisfactory accounting for the infant's reaction to involuntary social segregation.

**Separation effects and Cairns's contiguity analysis.** Cairns's hypothesis concerning the effects of the removal of the social object receives some support. He predicts that such removal will result in the disruption of the subject's routine responses, and this is certainly true in practice. However, as it stands, this prediction does not anticipate the protest-despair pattern we encountered in the literature. Further, as was pointed out in the critique of the instrumental/operant theory, separation results in more than the cessation of some responses – it disposes the organism to *engage* in other responses. For example, a number of writers have suggested that the protest phase involves something like a search for the lost object. Therefore, in its present form the contiguity analysis seems at a loss to account for the *emergence* of responses to separation.

**Separation effects and the opponent-process model.** The predictions of the opponent-process theory concerning separation are partially confirmed, to the extent that brief separation does lead to the occurrence of distress in all the species under consideration. But because the theory speaks only of a decline in the amount of distress over time (as the B-state diminishes), it cannot readily explain the separation-induced despair, depression, or general inactivity that follow the more active or agitated forms of emotional expression. The pattern of an initial stage of active upset that is supplanted by an inactive period of upset (followed by an eventual return to "normalcy") has been recorded in children, monkeys, and chicks. In fact, an overwhelming majority of reports have documented this trend (although exceptions have been noted).

These findings are problematic since the opponent-process model admits to only two hedonic states, A, and its qualitative opposite, B. As the opponent-process model of separation effects now stands, it takes this form (after Solomon & Corbit, 1974, p. 121): "normalcy" → A-state (attachment) → B-state (distress early in separation) → "normalcy". The data, however, indicate that there may be a third hedonic state – one quite different from both A and B. If the model were to include all the reliable findings concerning separation, it would appear as follows: "normalcy" → A-state (attachment) → B<sub>1</sub>-state (distress early in separation) → B<sub>2</sub>-state (inactivity later in the same separation) → "normalcy."

The question is, whence the B<sub>2</sub>-state? Is it another form of slave process to A, or might B<sub>2</sub> be enslaved to B<sub>1</sub>? To speculate further, might B<sub>2</sub> be a joint function of A and B<sub>1</sub>, or might B<sub>2</sub> actually be some C-state? We cannot say, and neither can we find the answers to these questions in the original model. Of course, the opponent-process theorists have addressed the issue of changes in A- and B-states over time, and they posit *quantitative* shifts in B as a function of repeated experiences of A (i.e., B becomes B'). However, we are speaking of apparently *qualitative* changes in B during a single separation episode, when multiple experiences of A could not be responsible.

## Conclusions

### General theoretical evaluation

Doubtless, some form of learning must be involved in the attachment process. Indeed, the operant theorist can point to the finding that the presence of a positive reinforcer causes proximity-seeking (shown in puppies, by Igel & Calvin, 1960), and the classical conditioning theorist can allude to the imprinting data showing that previously neutral stimuli (static features of an object) can exert control over attachment responses after association with an unconditioned stimulus such as movement (shown in ducklings by Eiserer, 1977).

Even so, learning theories are beset by an overabundance of problems in accounting for important attachment phenomena. There are not merely a number of points on which to criticize this interpretation, nor is it the case that the relevant data are lacking. Rather, the theory successfully explains few of the phenomena reviewed in this paper. As we have seen, learning theory makes predictions that are at odds with the empirical evidence on maltreatment, does not have a mechanism to account for secure base effects, and cannot convincingly account for separation effects. We are forced to conclude that learning theories of attachment are not particularly useful at this point.

The ethological theory has fared much better, since at least some of the data we have reviewed are consistent with its predictions. However, this theory has perhaps gone too far in stipulating the necessary and sufficient conditions for the formation of infantile social bonds. The ethologists propose that since the bonding process represents a biological disposition, it is most likely to emerge under biologically appropriate conditions, or in



an ordinarily expectable environment. Instead, we find the young becoming attached to things that bear very little resemblance to any biological being, much less the adult of the particular species, and it is clear that infants develop bonds to objects that do not respond at all. Additional questions were raised about whether an abusive object, to which certain infants certainly can become attached, can be conceived to fall within the limits of an ordinarily expectable environment for those species. The necessary and sufficient conditions for the formation of infantile attachments are not well specified by the ethological theorists, and this part of their theory warrants revision and elaboration.

Although it has shortcomings in the area of secure base and separation effects, the contiguity analysis holds some promise for an understanding of other aspects of the bonding process. It is the only analysis that accounts successfully for the formation of bonds under maltreatment. The provisions of this analysis can account for the rapid shifting of attachment under certain circumstances.

The opponent-process model does not go far enough. As originally conceived, of course, this theory was not meant to account for every feature or aspect of attachment, but only certain "affective dynamics" associated with the bonding process. To date, opponent-process theorists have applied the model to a surprisingly wide range of phenomena, including addiction, sexual appetite, and fear provoked by the diagnosis of terminal illness. But the mechanisms involved in these experiences can have little to do with the infant's experience of primary attachment, and one wonders just how far this theory illuminates the process of attachment. The theory takes for granted the basis for the A-state alleged to underlie the offspring's prosocial responses. This can be viewed as a serious omission. On the other hand, this theory represents a laudable rejection of the view that imprinting is an arcane or exotic process comprising an explanatory concept that is inapplicable to many species, including our own. The opponent-process analysis attempts to place diverse bonding processes in the same context and in the mainstream of motivational theory.

### Closing thoughts and recommendations

In the course of the literature review and theory evaluation provided above, we have pointed toward several areas where crucial data are lacking. It is not our intention to reiterate these issues here. Rather, we wish to focus on several themes that have been inadequately represented in past research, and that constitute significant lacunae in the field.

The first theme has to do with the infant's role in the attachment process. The question is: Are infants passive recipients of socializing stimuli that foster the formation of a bond, or is there some endogenous process that impels infants to become attached? Among precocial birds at least, there is compelling evidence that the infant plays an extremely active role in seeking experience with potential attachment figures. Bateson's (1976; Jackson & Bateson, 1974) data are especially impressive: he found that naive hatchlings in the presence of an attachment object worked (i.e., pedal-pressed) for the opportunity to view novel aspects of that object. Clearly, the birds were not passively learning the features of an object that merely presented itself to them (see also Rajecki et al., 1977, on the activities of chicks denied social contact).

The relevance of this issue is illustrated by several of the findings discussed above, and those having to do with maltreatment effects are especially pertinent. Researchers have demonstrated quite convincingly that infants develop attachments to abusive individuals – individuals who never provided either the positive responses that reinforcement theorists emphasize or the "appropriate" responses discussed by the ethologists. It is a phenomenon quite different from the enhancement of attach-

ment behavior by maltreatment after bonding has taken place – a prediction of the ethological theory that has earned empirical support. The data suggest, in sum, that infants may be impelled to form attachments regardless of (rather than because of) the adults' responsiveness to them. There is an urgent need for further attention to this issue. Although several workers (Ainsworth, 1969; Bell & Harper, 1977; Lewis & Rosenblum, 1974) have called attention to the infant's active role in forging its social relationships, it seems that this role may still be underestimated.

Our second point is that the theorists we have evaluated have all provided accounts of the attachment process that have cross-species utility. This is certainly a laudable feature. It is necessary, however, to take serious account of species differences as well, for we propose that an adequate account of social development depends on appreciating the unique adjustments or adaptations that face each species. Explanations that cannot proceed beyond generalities such as "adaptive significance," "survival value," and "stimulus control" are not sufficiently sophisticated by contemporary standards. Several recent findings illustrate the pervasiveness of species differences. For example, Sackett et al. (1976) have demonstrated that different types of macaques respond in markedly different ways to standard separation experiences. Similarly, Rajecki et al. (1978b) were unable to demonstrate in chicks the multiple peer separation effect (maturation arrest) that has been documented in infant and juvenile monkeys (Suomi et al., 1970). There are surely numerous other examples, but we simply wish to underscore the dangers inherent in adhering, implicitly or explicitly, to the notion that identical bonding processes need be involved in species as far removed from one another phylogenetically as precocial birds and altricial human infants. In short, we propose that an understanding of social bonding will emerge only when attention is paid to both species differences and species similarities.

Finally, we wish to redirect attention to a third basic issue – that concerning our assumptions about the emotional response experienced by the infant. For example, since the behavioral referents (i.e., distress) are similar and their supposed adaptive functions (i.e., signalling to the protective adult) are the same, can we assume that the infant responding to an abusing attachment figure and the infant responding to separation from an attachment figure experience the same emotion? Most interpretations would assume that the experiences are not similar, though no one has suggested how one might distinguish between the two in a manner that is not circular. Scott and Bronson (1964) have initiated research on the differential determinants of distress in pups, while Wolff (1969) has argued that there are at least four different types of crying in human infants. Such studies point the way toward the refined identification of different infant emotions and thus facilitate the development of sophisticated interpretations of the phenomenon of early infantile attachment – a phenomenon that, we must conclude, still lacks an entirely satisfactory accounting.

### NOTES

1. For a review of other interpretations of imprinting see Rajecki (1973).
2. Fisher (1955) was literally in a position to appreciate this particular treatment. He reports that both the experimenter and the subject were located on the shock grid for this procedure and both received the stimulation.

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### by Mary D. Salter Ainsworth

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**The Bowlby-Ainsworth attachment theory.** The ethological-evolutionary attachment theory formulated by Bowlby (1969 op. cit.), to which I have contributed, does not consist of a tight series of propositions in the mathematico-physical tradition, from which hypotheses may be derived and tested; nor can the theory be accordingly invalidated should any one of these hypotheses fail to meet an adequate test. On the contrary, this is an open-ended theory, intended both to help us to understand research findings to date and to provide guidelines for future research. The theory is clearly open to extension, modification, and refinement in the light of research findings. The theory fared relatively well in the review by Rajecki et al. and, on the whole, was dealt with understandingly. Nevertheless, in their attempts to fit theory with data, these authors have tended to use the former as a source of hypotheses to be tested against the latter, so as to point out certain inadequacies. Some of these seem to me rather to be inadequacies in their understanding of the theory. Others are well-founded and indicate the need for further research, which, of course, may be viewed as a heuristic consequence of the theory. It is my intention to comment on both types of inadequacy.

I have three major points, of which the first relates to the issue of the conditions necessary for an infant to form its first attachment – and this is indeed a complex issue (1). The second is concerned with the issue of cognitive constraints on attachment-formation among humans (2). The third, and perhaps least complex, deals with the "security-blanket" phenomenon (3).

1. It seems to me to be a misunderstanding of our attachment theory to infer that infants require a close approximation to what Bowlby terms the "environment of evolutionary adaptedness" in order to become attached. It appears that the bias toward attachment is so strong that an infant tends to become attached to someone – usually his principal caregiver – except under conditions that represent an extreme departure from what Rajecki et al. term "an ordinarily expectable environment." Even though Bowlby (1969 op. cit.) hypothesized that human mothers are predisposed to display "reciprocal maternal behavior" dovetailing with infant attachment behavior, and even though it seems reasonable to suppose that a mother figure responsive to infant behavioral signals is an important feature of the environment to which infant behavior is preadapted, there is nothing implicit in attachment theory that suggests that sensitive maternal responsiveness is required for infant attachment formation.

Rajecki et al. ask whether it is the amount of interaction with, or appropriate and sensitive responsiveness from, a potential attachment figure that is a precondition for attachment formation. This is a researchable issue. Since we must rely on experiments of opportunity, however, relevant research is difficult and, to date, unfortunately meager. Nevertheless, there is evidence that a gross insufficiency of interaction with a mother figure does prevent, delay, or hamper the development of attachment. Individuals reared throughout infancy in a severely depriving institutional environment tend to fail to become attached to their caregivers there, and, should the institutionalization persist beyond eighteen months or so, they find it difficult later to form attachments to foster mothers when given the opportunity to do so. Studies of caregiving arrangements in such depriving institutions have indicated that the relative paucity of staff and the way in which their duties are organized prevent any single infant from having more than brief snatches of interaction with any single caregiver, and these at widely-spaced periods (Ainsworth 1962). (Consequent upon insufficiency of interaction, however, is the fact that these brief snatches of interaction make it impossible for caregivers to respond to infant signals at all promptly, and even to detect any but the most conspicuous of them.) The clinical literature on "failure-to-thrive" in infants suggests that conditions of insufficient interaction may also be encountered in the home environment. These considerations have formed the basis of my identification of the amount of interaction between an infant and an adult figure as a condition for attachment formation. Just how much intervention is required for it to be *sufficient*, however, must still be determined.

Focusing on infant-father attachment, Lamb (e.g. 1976 *op. cit.*) has argued that it is the emission of appropriate and sensitive responses to infant behavior that is the important condition for attachment formation, rather than sufficiency (i.e. amount) of interaction. Pertinent to this issue is Bowlby's (1969 *op. cit.*) concept of "monotropy" – which would perhaps have been more generally understood had he merely written of a hierarchy of attachment figures rather than using the term "monotropy." He clearly acknowledged that a human infant could and usually did become attached to more than one figure. He held, however, that one of these was the primary attachment figure, the mother figure – the principal caregiver who played the mother's role, whether the natural mother or not. Other figures he conceived to be secondary and supplementary to the principal attachment figure. Among the Ganda (Ainsworth 1967 *op. cit.*), I found that such supplementary figures might have only occasional interaction with the infant, and indeed might be responsible for none of its routine care. In large extended families with many familiar figures, infants tended to become attached to only a few and showed sharp preferences. It seems likely to me that such preferences were indeed directed toward persons who were sensitively responsive to the infant, perhaps particularly in the context of playful interaction.

The crucial question, however, seems to be whether an infant could form its *first* attachment to a figure who was available only infrequently, however sensitively responsive the figure was, if during most of its waking hours the infant was so grossly deprived of interaction with its principal caregiver that no attachment could take place. No existing research provides an adequate basis for answering this question, although some of my 1967 Ganda data are suggestive. The sample of 28 infant-mother dyads contained four infants who did not appear to become attached to their mothers at the usual age. Their mothers left them in a crib (or its equivalent) most of the time, did not respond to crying signals, and gave very little attention beyond minimum routine care. In my opinion there was insufficient interaction to support attachment formation. Only one of the four showed any evidence of differential attachment behavior – a little girl who was reported to greet her father when he returned home twice a week. The observational data, however, are inadequate to allow me to judge whether any of these four had become attached to any figure – whether mother or other – during the period spanned by the study. It seems unlikely to me that an infant can form its first attachment to a figure with whom it has very infrequent interaction, however sensitively responsive that figure may attempt to be.

Relevant to this issue is the finding that chicks can become imprinted on styrofoam objects, and that Harlow's rhesus infants became attached to cloth surrogates – in both cases objects that were capable neither of interaction nor of sensitive responsiveness. In this context Rajecki et al. properly raise the issue of the importance of the infant's own activity in becoming attached. In comparison with chicks and even rhesus infants, the human infant is very much more helpless for some months after birth. Without the cooperation of

an animate caregiver there is no way in which the infant can achieve the contact and/or proximity required to terminate its attachment behavior. Signals directed toward either an inanimate object or an unresponsive human caregiver receive no feedback, and consequently the infant experiences no feeling of efficacy as a consequence of its behavioral signals. It tends to become apathetic and passive, but it also tends to be delayed in the development of the more active behaviors that might otherwise enable it to take the initiative, as does the infant of one of Harlow's rhesus "motherless mothers." There is no evidence that a human infant can form its first attachment to an inanimate surrogate; it is too handicapped by its own ineffectiveness in gaining and maintaining proximity/contact on its own account.

Rajecki et al. properly report Bowlby (1969 *op. cit.*) as implying that an attached infant would be strongly drawn to its attachment figure under conditions of stress, even if the attachment figure itself were the source of stress. This "prediction" was based in part on findings of Harlow and his associates with rhesus monkeys, but also in part on clinical reports. It is well known among clinicians that young children removed from "bad" mothers may nevertheless be severely disturbed at the breach of the bonds they had established with them. In my own research sample it was clear that infants can and do become attached to mothers who are rejecting, interfering, and/or ignoring. All such mothers may be described as relatively insensitive to infant signals and communications. Their infants may be identified as attached, albeit anxiously attached.

In some anxiously-attached infants there simply seems to be a lack of confident expectation that the mother will be accessible and responsive when especially needed. In others this anxious condition is complicated by a severe approach-avoidance conflict with respect to close bodily contact, apparently consequent upon maternal rebuff and/or other painful experience in the context of such contact. Under conditions of stress, when attachment behavior is intensely activated, this approach-avoidance conflict leads paradoxically to avoidance of the mother under circumstances in which other attached infants strongly seek to gain and maintain contact (Ainsworth, Blehar, Waters & Wall, 1978).

These findings within a "normal" sample of white, middle-class one-year-olds have relevance to the issue of attachment among young victims of child abuse. To our knowledge there has been only one study specifically directed toward observation of the attachment behavior of abused infants, and even this one was concerned with behavior to caregivers other than parents. George and Main (1978) compared a sample of ten abused children, aged between one and three years, with a carefully matched sample of ten infants "under stress" but not abused. Both groups were observed in the special daycare centers that had been established to care for infants and young children at risk. The findings suggest that the abused infants behaved toward their day caregivers in much the same way as our rejected infants behaved, under stress, toward their mothers – with signs of an approach-avoidance conflict and a tendency to avoid the caregiver under circumstances in which most children would be especially receptive to her. The implication is that the abused child tends to be capable of attachment behavior, and indeed attachment, even though there is more avoidance, aggression, and indications of conflict than with the non-abused child.

There is good reason to believe that abusing parents have themselves had physically aggressive and punitive childhood experiences (Parke & Collmer 1975). Thus the parents may be described as having undergone a developmental anomaly, as Rajecki et al. suggest. However, this does not rule out the possibility that a young child's interaction with such parents may also lead to anomalies in its own development. In summary, this first cluster of issues raised by Rajecki et al. seems to imply nothing incompatible with the Bowlby-Ainsworth attachment theory, but it does nevertheless indicate the need for much further research.

2. Rajecki et al. comment that neither Bowlby nor I have explained why cognitive constraints affect the bonding of human infants to their mothers but do not retard bonding in nonhuman species. They do acknowledge that there are species differences in attachment phenomena. It is strange that they do not also acknowledge the possibility that differences between humans and other species might rest on differences in cognitive function and development. To discuss the issue further we need to consider responses to separation as they relate to attachment.

Bowlby (1960 *op. cit.*) suggested that separation distress is inextricably intertwined with attachment – and indeed, studies of nonhuman species sup-

port this suggestion. Nevertheless it is true that infants who have become attached do not invariably protest the departure and/or absence of an attachment figure. There are at least two major considerations here. First, response to separation is affected by the expectations that an infant has built up concerning the accessibility and responsiveness of its attachment figure. These expectations form an important basis for the development of a "working model" or "internal representation" of that figure, and they clearly imply "cognitive constraints" – including the concept of the continued existence of the attachment figure when not present to perception. Although I cannot go into detail here, it seems clear to me that the nature of such expectations accounts for different responses to separation under different conditions – for example, the fact that separation is more likely to be protested in an unfamiliar as opposed to a familiar environment; that "major" separations, such as those studied by Schaffer & Callender (1959 *op. cit.*) and by Yarrow (1967 *op. cit.*), more dependably activate protest than the very brief separations with which most recent separation research has been concerned; and that infants securely attached to their mothers are less likely than anxiously attached ones to protest brief, everyday separations in a familiar environment (Ainsworth 1967 *op. cit.*; Ainsworth et al. 1978).

Indeed, age differences, even slight ones, affect the likelihood of separation protest (Stayton, Ainsworth, & Main 1973 *op. cit.*; Mahler 1968 *op. cit.*; Marvin 1977). Furthermore, it seems likely that cognitive differences between humans and monkeys account for the fact that humans are capable of the defensive process of "detachment," whereas monkeys apparently are not. In short, it is impossible to adequately account for the diverse phenomena of attachment and separation in humans without taking cognitive processes into consideration. Perhaps the extension of the imprinting model from precocial birds to other species has led researchers to overlook the possible rewards of exploring the effect of cognitive processes on attachment/separation phenomena in mammalian species, and especially in nonhuman primates.

A second consideration is that the presence of a secondary attachment figure may well affect the occurrence or intensity of a child's protest upon separation from the principal attachment figure (or, indeed, vice versa). Research such as that of Robertson & Robertson (1971 *op. cit.*) suggests that sensitive care by substitute parent figures may substantially reduce the severity of separation disturbance. These findings do not necessarily imply that attachment figures are altogether interchangeable, nor that attachment to the primary attachment figure is undone by sensitive substitute-parenting. Furthermore, the difference between pigtail and bonnet monkey infants in response to separation from the mother suggests that species other than human may have attachments secondary to infant-mother attachment. It is clear that further research is needed – research into the relation between attachment and responses to separation, as well as into the notion of a hierarchy of attachment figures.

3. Finally, let us consider the issue of an infant's attachment to an inanimate object such as a security blanket. Let us note that this issue is concerned with supplementary or secondary attachments rather than the formation of a first attachment. Infants so deprived of interaction with their caregivers that they fail to become attached to any of them apparently do not become attached to inanimate objects. Why is it that some (but by no means all) infants who have become attached to the mother figure form supplementary attachments to cuddly toys and favorite blankets?

In conjunction with my Ganda study (Ainsworth 1967 *op. cit.*) I formulated a theory that seems relevant. This theory rests on the fact that once an infant can actively gain and maintain proximity/contact with an attachment figure on his own behalf, he can do so through a variety of specific behaviors. That there is a certain redundancy of such behaviors suggests a fail-safe mechanism. Some of these behaviors become closely tied into his relationship with his principal attachment figure, but some of them may become splintered off and either cease to serve the attachment system or become directed toward other objects of attachment.

Such a notion was prompted by the finding that infants who were permitted to seek the breast whenever they wished it, often merely for comfort, were disturbed by weaning, whereas infants who could gain the breast only upon maternal initiative showed minimal weaning disturbance. It was as though the former infants integrated sucking behavior into the system of behaviors that mediated attachment, so that when the breast was finally denied to them at weaning, it was as though they were suddenly rejected by their principal attachment figure, making the attachment relationship clearly anxious in quality. On the other hand, infants who were not allowed initiative in gaining

the breast seemed to have their sucking behavior splintered off from the attachment system, serving only the food-seeking system. Since weaning took place only after other modes of food intake and acceptance of a variety of foods had been well-established, it did not disturb the infant – and certainly it did not seem to it to constitute rejection by the attachment figure.

Rowell (1965) has reported findings that are congruent with this theory. She reared an infant baboon, feeding him by bottle and providing him with a pacifier for comfort. He became attached to her, and although she could not hold him nearly as much as he wished, when she was available he clung to her. When she was not available he turned to his pacifier for comfort. When hungry, he searched for his bottle and eschewed the pacifier, just as he ignored the bottle when his attachment behavior was activated. In this case it appeared that sucking behavior was splintered off from the system of behaviors that mediated attachment to his principal attachment figure; the pacifier was a supplementary object that provided solace under stress; the bottle was exclusively tied into the food-seeking system.

The security-blanket phenomenon resembles the infant baboon's pacifier. If the mother figure, to whom the infant is attached, is available when attachment behavior is activated, he seeks contact with her. When she is not available – perhaps because of a policy of leaving him to cry at bedtime or naptime – he may cling instead to his blanket, which is available to him. In time the blanket becomes an attachment object, offering security when the mother is unavailable. However, it must be emphasized that only infants who have become attached to a mother figure seem to be able to avail themselves of such a substitute; indeed, it may well be that only infants who are securely attached can do so.

Despite all of these considerations, I wish to congratulate Rajecki et al. for a careful and provocative review, which should stimulate further research. From the point of view of the particular version of ethological attachment theory to which I subscribe, stimulation of further research is indeed the most important result.

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by N. Bischof

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**On the necessity of "appropriate behavior" on the part of the caregiver.** In response to the article by Rajecki et al., I would like to limit myself to commenting on an argument against the ethological theory of attachment. Ethologists assume that infantile motivation in an "expected environmental niche" leads to behavior that optimizes the child's chance of survival. "Expected niches" are those that are similar to the environment in which the species evolved. The authors state that under experimental conditions attachment forms not only to parents, but also to inanimate objects bearing no similarity to conspecifics, and even to objects that severely maltreat the child. According to the authors, this poses a serious problem to ethological theory, since one cannot possibly view these objects as constituting part of an ordinarily expectable environment.

This argument contains a fallacy. Suppose that the objects constituting the "expected environment" of an infant immediately after birth or hatching dif-



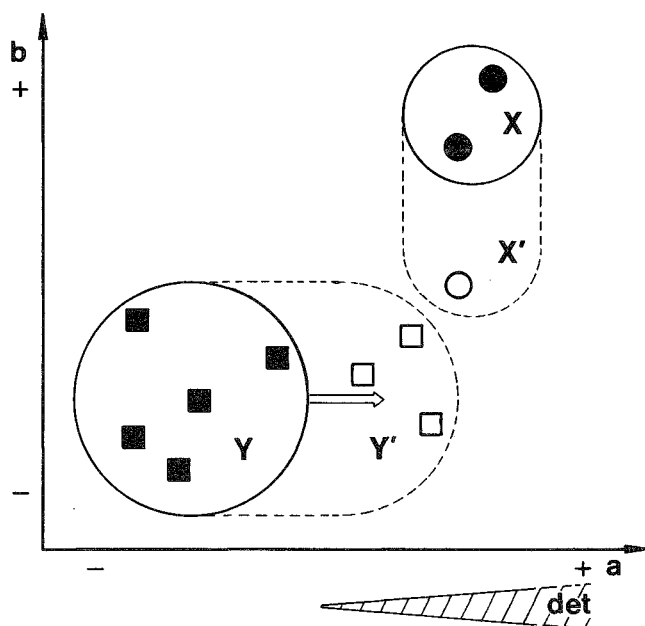


Figure 1 (Bischof).  $a$  = average intensity of attention-attracting stimuli emitted by object;  $b$  = readiness of object to behave prosocially to infant;  $X$  = parents;  $Y$  = other objects;  $X'$  = "bad" parents;  $Y'$  = other objects becoming more conspicuous in later stages of ontogeny;  $det$  = detector for object conspicuousness, releasing following-response in an early ontogenetic stage.

fer, among other characteristics, in two dimensions,  $a$  and  $b$ , where  $b$  is the probability with which the infant can expect prosocial behaviors (for example caregiving) on the part of the object, and  $a$  refers to the average intensity with which the object attracts the attention of the infant.

We may assume that the "expected" objects fall into two classes (see Figure 1): parents and their possible helpers (Class  $X$ ), and other objects (Class  $Y$ ). The infant can only expect prosocial behavior ( $b +$ ) from objects  $X$ ; all other objects are at best unconcerned, but they are frequently even hostile and dangerous ( $b -$ ). At the same time it is the objects  $X$  which regularly come closer to the newborn than all others and interact with him more, and which thus score highest on the scale of conspicuousness ( $a +$ ).

In order to stay in proximity to prosocial objects, the infant ought to be able to detect them. The initial cues for this detection must be simple enough to be accessible without previous learning. In an "expected niche" in which neither unfamiliar conspecifics, nor other animals, nor inanimate objects are as conspicuous as the parents, a stimulus detector sensitive to high grades of  $a$ , as indicated in Figure 1, would be completely sufficient. It would cause the infant to attach to those objects in his early environment that are most efficient in attracting his attention.

Such a mechanism is of course expedient only in an early phase of ontogeny. Discriminating cues in other character dimensions ( $c$ ,  $d$ , etc., not represented in Figure 1), concerning the individual physiognomies of the parents, should become associated with the unconditioned stimulus  $a +$  as soon as possible. The older the child, the greater the probability of his encountering unfamiliar objects ( $Y'$ ), and then the simple detector system in Figure 1 no longer makes correct discriminations. The sensitive period of imprinting, therefore, should be limited to early ontogeny, and this is generally the case.

Precisely when primary detection of potential caregivers is achieved through a mechanism such as that in Figure 1, we find the infant in the laboratory attaching himself to any surrogate, as long as this surrogate fulfills the criterion of predominant conspicuousness. Rajecki et al.'s imputation that the ethological theorist is forced to conclude that non-specific objects belong to the "expected environment" of the species is positively incomprehensible. Exactly the opposite is the case: had styrofoam cubes, toy trains, flashing lights, or even potential predators been regularly present in the intimate environment of early childhood in the course of evolution, then natural selection would have presented a mechanism of the kind discussed

from ever developing – the danger of a child's following an object prone to neglect or kill him would have been too great.

What about abusive objects? If they belong to class  $Y$  (e.g. predators), we cannot expect the imprinting mechanism to be able to react to them in an adequate way. If such objects do manage to approach the child so as to activate the detectors of the attachment system, the child is done for anyway. Whatever measures natural selection has produced to counteract infant predation, they must prevent predators from lingering in the lower-right quadrant ( $Y'$ ) of Figure 1. That is, predators must be chased or lured away, or the infants must be concealed. Since this is what the attachment system can be expected to presuppose, anecdotal reports of occasional imprinting to predators in captivity do not pose any theoretical problem.

If abusive objects are pathological parents ( $X'$ ), this again would have hardly been able to force the evolution of the detector system in another direction. Parental maltreatment no doubt reduces the child's chances for survival, but the child normally has no alternative; there is no one to whom he can run and be better cared for. In extraordinary cases the helpful she-wolf might turn up to nurse Romulus and Remus, but extraordinary cases have no effect on evolution. It is therefore always best to stay with one's own parents, who are generally at least somewhat care-oriented and not quite as uninhibited in inflicting harm as strangers.

Thus, Lamb's theory, that infantile attachment presupposes "appropriate behavior" of the parents, may be based on sensible arguments elsewhere, but it is not implied in the context of ethological theorizing. If this assumption appears to be invalidated by empirical results, Lamb should certainly not attribute the error to his ethological position.

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**Is there adaptive significance in the persistence of infantile attachment to maltreating attachment figures?** The Rajecki et al. paper presents a valuable and (almost always) fair organization and description of several aspects of infantile attachment in four animal species. The basic premise – that theories seeking to explain infant attachment should be able to account for maltreatment effects, secure base effects, and reactions to involuntary separation – is reasonable. However, although such theories should be able to account for these phenomena, they don't always involve specific predictions in the areas reviewed. It was therefore necessary for Rajecki et al. to derive predictions, make assumptions, and otherwise amplify so as to make contact between theory and phenomena. Such a situation presents bountiful opportunity for the construction of straw men, and it seems to me that the authors have restrained themselves admirably.

Although their conclusion that "learned theories of attachment are not particularly useful at this point" is bound to raise a few hackles, it seems justifiable in terms of the information presented. I might go a little further, however, and add both the contiguity analysis and the opponent process model to this same category; none of them alone can begin to cover the range of attachment phenomena outlined. Moreover, there is no straightforward combination of these positions which suggests itself as an adequate treatment of these attachment phenomena.

The "Bowlby-Ainsworth" ethological theory, which loses exclusive identification with Bowlby and Ainsworth somewhere along the line, is obviously more complete and more relevant, coming as it does in large part from observations of attachment phenomena in human children. However, Rajecki et al. conclude that this, too, is not totally satisfactory, because it has "gone too far in stipulating the necessary and sufficient conditions for the formation of infantile social bonds."

There is something decidedly odd about this conclusion. The only real problems cited for the ethological model stem from a position that attachment depends on appropriate and sensitive responses by the attachment figure to the infant's reactions. This position encounters considerable difficulty in explaining maltreatment effects, since all species studied do show some (or even much) attachment to maltreating attachment objects, at least when there is no alternative and less maltreating attachment figure available.

This problematic statement is neither from Bowlby nor Ainsworth, however, but comes from earlier work by one of the present authors, Lamb (1976, 1978 *op. cit.*). So, strictly speaking, the Bowlby-Ainsworth ethological model holds

up very well indeed, while it is Lamb's addition to this model which might be viewed as unfortunate in terms of the data presented.

Rather than suggesting that the Lamb statement be disregarded as irrelevant to the Bowlby-Ainsworth formulation, however, I would like to suggest that its difficulties may be more apparent than real. The basic point is that "maltreatment" is sometimes an appropriate and sensitive reaction to an infant's behavior; infants do misbehave on occasion, and appropriate parental behavior, especially in higher species, contains a systematic disciplinary or punishing element. This implies an adaptive significance in the maintenance of infantile attachments to punishing figures. It might be argued, of course, that the maltreatment involved in the studies cited was not appropriate to the infants' behaviors, but this line of reasoning is without force unless the infant is able to infallibly distinguish which of its behaviors should be punished and which should not. I am unwilling to take so dim a view of the potential value of education in any higher species.

The optimal attachment figure for a human infant (on which the ethological model was based) is one which shows a great number of prosocial behaviors toward the infant, but sometimes provides disciplinary and punitive responses. Although human experimentation is obviously lacking in this area, it is notable that dog pups that are both indulged and punished show more attachment behaviors to their handler than do pups that are only indulged (Fisher 1955 *op. cit.*). For human children as well it seems a plausible hypothesis that total indulgence leads to lower levels of prosocial behavior to the parent (why seek what is never denied?) as well as to higher levels of misbehavior.

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*Three scientists in search of a theorist (apologies to Pirandello).* In a twist on the old saw, Rajecki et al. have bravely climbed out on several limbs, thus enabling others to cut those limbs off behind them. Applying theories to phenomena other than those to which they were originally addressed is a powerful technique for comparing theories, and the authors are to be admired for their comparisons of attachment theories. In making such comparisons, however, they risk making predictions which may sound foreign to the original theorists (at least some do to me), and they have not considered the most recent and detailed formulations of some theories. Since those theorists will presumably make their own comments, I will briefly discuss some more general conceptual problems with Rajecki et al.'s critique. These problems are neither novel nor limited to their analysis.

In any comparative analysis, selection of species and the bases for species comparison are difficult problems. Rajecki et al. base their analysis on comparison of four groups of animals but provide no rationale for comparing those groups. Granted, most information is available on these, but that is not a criterion for expecting similarities across those groups either in terms of attachment behaviors or their underlying mechanisms. There is no common phylogenetic origin, social ecology, or developmental trend on which to base comparisons. The question is: Why should similar manipulations be expected to produce similar attachment behaviors in these groups? The similarities, across species, of some attachment behaviors are striking indeed, but similarity in some does not justify expectation of similarity in all.

The emphasis on nomothetic group characteristics tends to downplay important within- and between-species or population differences. Thus, we have the precocial bird, the dog, the monkey, and the human – a tendency which Hodos and Campbell (1969) and Lockard (1971) have so acutely criticized as a general problem for comparative psychology. Consider briefly "the monkey." Most research on social behavior in monkeys has been on rhesus, and the contributions of that research to our understanding of social development are manifest. But the selection of the rhesus appears to have been on the basis of convenience, not rational consideration. Like other subjects in psychological research, such as the albino rat and college sophomore, it was simply there. (Whether the albino rat, the rhesus macaque, or the college sophomore is a better model for human behavior is a matter of some dispute.) Is the behavior of rhesus more relevant to an understanding of attachment than that of other monkeys? The important interspecies differences reported by Rosenblum and Kaufman (1968 *op. cit.*) and Sackett et al. (1976 *op. cit.*) are described, but the implications of those differences for theories of attachment are considered only briefly. Particularly exasperating is the

statement, "One *reliable* finding is that infant monkeys that have been segregated from their mothers initially protest . . . and then fall into a kind of despair or depression. . . ." [emphasis added], which is followed a paragraph later by a description of the marked differences in behavior between bonnet and pigtail macaques upon separation from the mother! Pigtails show the "reliable finding," while bonnets simply hop onto another adult in the family group (Rosenblum & Kaufman 1968 *op. cit.*).

Individual intraspecies differences in attachment also tend to be large, not only in humans, in which different patterns of infant behavior may interact with those of the attachment figure, but also in birds, in which the imprinting object is inanimate and unvariable. Thus different attachment types have been noted in human infants (Ainsworth 1967 *op. cit.*) and in chicks towards an imprinting object (Fischer 1970 *op. cit.*; Hall, 1976; Hall & Brown 1977). As Rajecki et al. noted, some human infants become attached to inanimate "cuddlies" such as blankets, whereas others do not. Such differences may imply a larger role for congenital differences in infants than has generally been acknowledged in attachment theories (Hall & Brown, in preparation), leading to an analysis of attachment similar to that of Thomas, Chess, Birch, Hertzog, & Korn (1963) in their study of development of temperament. Those attachment theories which stress interaction, particularly from an epigenetic standpoint (e.g. Cairns 1972), would seem best able to handle these differences.

Cross-species comparisons frequently lead to conceptual and definitional problems. Clear operational definitions of the phenomena discussed by Rajecki et al. are missing. They do not even define attachment. (I do not blame them!) This is a particular problem with maltreatment, where the manipulations discussed under that heading may actually be loading on other variables. Anthropomorphism seems at work: If a procedure sounds aversive, it constitutes maltreatment. Thus, Rajecki et al. treat presentation of cold or shock in imprinting studies as maltreatment, although functionally such variables may be affecting arousal. Indeed, cold, shock, and other similar stimuli may increase attachment behavior by increasing arousal. Imprinting is particularly perverse, since either too low or too high an arousal level may evoke behavior that appears to be sleep, and it should be no surprise that imprinting is low in sleeping subjects. Andrew (1964) suggested that the different chick calls that have frequently been used as indicators of pleasure or distress are actually on a continuum, with small changes in stimulus contrast leading to "pleasure" calls, and large changes, to "distress" calls. Thus, Rajecki et al.'s conclusion that ". . . no procedure involving *pain* has prevented the formation . . . of some degree of attachment in precocial birds. . . ." [emphasis added] is questionable. Until maltreatment can be clearly defined and differentiated from arousal and other variables, interpretations must be guarded. The confounding of maltreatment and arousal may lead to serious misinterpretation of the studies themselves, and from there, since analogous manipulations are assumed to have analogous effects on all species discussed, to errors in cross-species comparisons.

Finally, more careful attention needs to be paid to the effects of the context in which attachment and changes in presence of an attachment figure occur (Bateson 1966 *op. cit.*; Cairns 1966, *op. cit.*, 1972). Organisms learn and become attached to settings, as well as to attachment figures (e.g. Brown & Hamilton 1977), and respond to changes in both. Context may be of paramount importance in understanding the results of a variety of attachment phenomena, particularly separation. Reminiscent of Lehrman's (1953) classic criticism of ethological deprivation studies, the question is not "has the infant been separated?" but "from what has it been separated?" The differences between pigtail and bonnet in response to separation from the mother appear attributable to differences in the context in which the attachment occurred in the first place. It is well to remember, also, that some human infants actually *benefit* from separation from the parents and home settings.

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**Beyond attachment?** It's about time for a reconsideration of the origins of social preferences. It has been over twenty years now since some of the plausible myths of behavior theory were challenged by simple but influential laboratory studies with monkeys and precocial birds, and about 15 years since systematic work began with human infants. The theories that were proposed then – including mine – were formed on an inadequate data base. It is mildly surprising to see that they are still considered to be viable, if not wholly virile. My question is this: Why hasn't the research over the past decade led to an elimination of the original statements, or at least to their drastic revision and extension? I will return to that matter shortly, after some comments on the Rajecki et al. review and its implications.

The strategy of the review is straightforward and defensible: identify the theories that have been employed in research, establish criteria by which they can be evaluated, and then see how each of them stacks up against relevant empirical findings. The success of the undertaking can also be readily determined. It hinges on (a) how well the theories were described (and understood); (b) the adequacy of the criteria that were established (why these three and not others?); and (c) the accuracy of the search for relevant evidence (was the search biased?). I will briefly comment on each of the above points.

First, the authors are to be congratulated for having plowed through the obscurities contained in most of the theoretical statements and to making some reasonable sense of each of them. If my comprehension of the models is any guide, the descriptions were tolerably accurate as far as they went. I fear, however, that they did not go far enough for some of the models. Hence they omitted the great emphasis placed on developmental variables by some theorists (by, say, Schneirla and me) and on interactional factors by others (by Ainsworth, Gewirtz, and me, and which constitute one of the few features that these statements have in common). I was especially curious as to why my companion theoretical paper was omitted, especially since it was published sufficiently long ago to have been incorporated, and I consider the two to be inseparable (Cairns 1972). The second paper spelled out why I considered any "attachment" theory that failed to give explicit attention to the integration of psychobiological and social learning processes to be wholly inadequate. The point seems to have been independently reached by the present authors.

Second, are the empirical criteria that were used to evaluate the models justified? I think so, insofar as they go. Again, the problem seems to be one of omission rather than commission. I was puzzled by the failure to include attention to the *diminution* of attachment bonds as the young grow older. Social preferences are not only formed, they are diminished (in most species) when another litter is born, or when the young are weaned. Since this phenomenon of "detachment" is as ubiquitous as "attachment," it seems reasonable to ask how well extant theories deal with the problem. My impression is that they do not handle the question very well (possibly because of the inattention most models give to developmental changes in general). Another criterion that seems relevant concerns the question of how new social preferences are formed. Studies in my laboratory (and elsewhere) suggest that fresh "bonds" are established readily and rapidly in most species. We observe, in three of the species cited, altogether rapid "attachment" formation: in chickens (Einsiedel 1975 *op. cit.*), in dogs (Cairns & Werboff 1967 *op. cit.*), and in human

infants (Fleener 1972). How do the various models account for the extension of the social world of the young, even when age is held constant? In any case it appears that the criteria selected may not have been the most powerful for the purpose of discriminating among the theories of attachment, although they were useful and informative.

The third point has to do with the literature cited. Obviously there was a need to be selective. All in all, the representation was fair, given the restrictions that were required. But there was one major instance where the citations appear to have been unfortunately biased, and I believe that the record should be corrected. I have summarized elsewhere the effects of separation (long-term and short-term, see Cairns 1977, 1979). From an overview of the entire literature it is not clear whether the deleterious effects of separation on health and well-being are the exception or the rule. I will not detail this controversial point here, except to note that the literature remains today highly controversial. I hope that further discussions of the matter will not present a one-sided view but will take into account the negative, neutral, and adaptive processes triggered by separation.

Now to return to the question that I raised earlier. Why hasn't there been more theoretical progress in the light of the intense interest and prodigious research of the past ten years? My answer is that there has been considerable progress made in understanding the developmental and interactional processes that give rise to social and nonsocial preferences. This includes most of the phenomena that have been called "attachment." The problem remains that "attachment" has been reified – despite protests to the contrary – so that a descriptively useful but theoretically vague idea has been assigned a role of nuclear importance in the explanation of psychopathology and the effects of early experience. The empirical basis for the reification and for the linkage between early social preferences and later personality disruption remains highly speculative. Perhaps if attention is redirected to the processes that govern the outcomes that have been "explained" by attachment, we will achieve a more powerful account of both the processes and the outcomes.

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**Ethological theory and infantile attachment.** Rajecki et al.'s paper performs a valuable service in reviewing current theories on infant attachment in the light of the now substantial data on mother-infant behaviour. My comments are restricted to the Bowlby-Ainsworth ethological theory, which figures prominently in the paper and, at the end, emerges as, at worst, no more deficient than several of the other theories discussed, and, at best, less deficient than most of them.

My main concern is whether the Bowlby-Ainsworth view, as outlined by Rajecki et al., properly constitutes a theory, in the sense of being a homogeneous set of premises and inferences from those premises. I doubt whether it does, and whether it is therefore appropriate to talk about "the ethological theory" as though it were a single explanatory concept or model. Reading through Rajecki et al.'s paper, it is possible to detect several strands of this theory. At the risk of crude oversimplification they are as follows:

1. Infants are born with a biological predisposition to seek proximity to and contact with conspecific adults.
2. Motor behaviours, such as clinging in nonhuman primates, and signals such as crying in humans, facilitate adult-infant proximity.
3. Such behaviours are advantageous and have been selected for in evolution.



4. Adults have a biological predisposition to remain close to and protect infants.

5. Given either a certain quantity or a certain quality (according to two different schools of thought) of interaction between an infant and a specific adult, a bond will form between them.

6. Discriminating attachment to a specific individual can be explained by four principles.

7. Separation of an infant from a figure to which it has become attached results in the infant's giving distress signals in an effort to retrieve the figure. Grief or depression result after prolonged separation, when the infant realizes that the attachment figure is not returning. Recovery from grief may thereafter follow, and new social bonds be formed.

8. There is a degree of proximity to an attachment figure which an infant deems satisfactory. This is the set-goal, and it can vary with factors both internal and external to the infant.

9. Infants are predisposed to be influenced by the expectable features of the social environment.

It is clear that the items in this list are heterogeneous, both in the types of phenomena they try to explain and in the kinds of explanation that they offer. The problems to which 1–9 (above) address themselves are as follows:

- i. What causes an infant to seek proximity?
- ii. How does it achieve such proximity?
- iii. Why should such proximity-seeking have evolved?
- iv. What causes adults to seek proximity?
- v. What conditions are necessary and sufficient for bond-formation between an infant and an attachment figure?
- vi. By what processes does bond-formation occur?
- vii. What is the effect of separation from the attachment figure?
- viii. How can one explain temporal variations in proximity-seeking by the infant?
- ix. What relationship is there between the factors that affect infant attachment and the factors that an infant normally encounters during its life?

The kinds of explanations offered are equally diverse. Some are primarily ontogenetic (items 1, 4, 5, and 6), some deal with the immediate causes of behaviour (6, 7, 8, and 9), some deal with the immediate effects of behaviour (2 and 7), and one deals with its evolution (3).

I believe that it is therefore incorrect to argue that there exists at present either "an ethological theory" or "a body of ethological theory" to account for mother-infant behaviour. Rather, there exists a set of diverse questions concerning mother-infant relationships and an equally diverse set of explanations attempting to cope with these questions. This is not to attack Rajecki et al.'s valuable summary of ethological research into mother-infant behaviour, still less to attack the ethological research itself. Rather, it is to argue that the contribution of ethological research needs to be evaluated at its correct level.

I wish to make two further points about the ethological model discussed in the paper. First, the dividing line between empirical fact and interpretative theory often appears to be very thin; occasionally the theory seems dangerously similar to the facts that it is trying to explain. This in turn can lead to circular arguments. Item 7 in the above list illustrates this. A human or monkey infant (of some species, at least) that is separated from its attachment figure initially shows signs of distress, then becomes quiescent and withdrawn (the phase of "anaclitic depression"), and then starts to form new bonds. This is description, not explanation. The explanation for these observations appears to be that the infant's faith in its attachment figure is shattered, and that an internal state (grief) develops that is responsible for its withdrawn behaviour. With time this wears off, and new bonds are formed. The prediction from this theory is that involuntary separation of the infant from its attachment object should lead to protest, followed by grief, followed by gradual formation of new social bonds.

The circularity is clear. I believe that if the value of ethological research into mother-infant behaviour is to be fully realized, then the distinction between hard data and interpretative theory must be kept perfectly explicit.

Finally, I would question the authors' criticism of item 9 above. Ethological theorists, they argue, suggest that infants are predisposed to be influenced by the expectable features of the social environment. Research shows, however, that infants can develop attachments to quite inappropriate objects, such as blankets or pathologically aggressive mothers, that one would

not expect to find under natural conditions. This aspect of ethological theory would therefore seem to be incorrect.

I believe there is an error of logic here. If, for example, the combination of warmth and softness is a necessary stimulus for infant attachment, then one should not be surprised that objects providing that stimulus, whether naturally-behaving or not, evoke attachment behaviour in the infant. A problem would certainly arise for this particular ethological theory if one were to find that an object possessing all the features of its natural mother failed to evoke attachment behaviour in a normal infant. I do not believe that such a problem arises when one finds that objects possessing some of the features of natural mothers can themselves evoke attachment.

## by Herman Dienske

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*The parental bond and the game of theorizing.* Indulgence in theorizing is profitable if it yields predictions that guide research planning. If the guide is followed, the resulting empirical studies can be used to dismiss or critically applaud theories. This second step was skillfully taken by Rajecki, Lamb, and Obmascher. Subsequently, the circle closes if a new or, more likely, an adjusted and better integrated theory emerges. The first word in the title of the article by Rajecki et al. correctly indicates that their turn of the theory-practice spiral is not fully completed.

The conclusion that ethological theories better and more completely account for three crucial aspects of infantile attachment than learning theories seems well elaborated. In a way this is not astonishing. Somewhat roughly put, ethologists often build their theories on a wide variety of naively but keenly observed phenomena, so that their theories cover broad experience, whereas learning theorists base their elegantly simple theories on ingenious but behaviour-restraining experiments. That the latter method led to a falsification of their own principles in the particular cases described by Rajecki et al. is a strong and important point. Of course, the authors did not consider rejection of learning theories, but one is left wondering about their appropriate place in the parental-bond theories.

Three examples, not so well documented as the three corollaries chosen by Rajecki et al., may serve to outline the relationships between various behaviour theories.

Rhesus mothers reduce body contact with their infants by breaking it or by preventing the infant from making contact (Hinde & White 1974). This is explained by another ethological theory – viz. on parental investment (Trivers 1974) – which predicts conflicts between parent and infant. Different mothers each have a fairly individual repertoire of contact-reducing acts. A developing infant soon learns to react to the particular repertoire of its mother – a clear example of "natural" operant conditioning. There is a complication that makes one think of a modified opponent-process theory: rejection of the infant when it is about to sleep leads to temper tantrums, whereas rejection when it is fully awake does not (Dienske, van Luxemburg, & Metz 1978, and unpublished results). Here we need at least two different states, in the sense of opponent-process theory, of which the postponed sleeping state is the unpleasant one. In the example, three different theories need to cooperate in an account for the observed behaviour.

If primates grow up isolated or under lack of parental stimulation, their parent-directed behaviour may become directed to substitutes (e.g. clasping, digit-sucking; Ozturk & Ozturk 1977), and they may change in external appearance (e.g., locomotion to a parent may be substituted for by body-rocking; see Dienske & Griffin 1978). This confirms the interpretation of Rajecki et al. that the infant may start with and work for its own attachment, as predicted by ethological theory. This theory, however, does not predict that these aberrant behaviours would continue if the initial lack of parental stimulation is alleviated. Yet this seems to be a common psychiatric experience. In family life a child thumb-sucking while holding its favorite cloth, but seated on a parent's lap, is a striking example. Reduction in strongly developed rocking and head-banging has been achieved after extinction or by strong negative reinforcement (Lovaas & Simmons 1969). In the case of substitute behaviour, ethology accounts for appearance, and learning theory, for persistence or disappearance, so that, again, the total set of phenomena can be better understood after integration of theories.

A major point in the area of infantile attachment, underrated by Rajecki et al., is the course of development and its relation to the quality of interactions. Poor quality of parental behaviour, including neglect, may lead to antisocial

behaviour, especially in boys (Rutter & Hersov 1977). This may become a major method to elicit the attention of parents, although the nature of the attention is punitive. Apparently punishment is preferred over neglect, which again supports the ethological notion that a child is active in shaping its bond, even if punishment scores higher than reward. However, it is inconsistent with operant conditioning theory. Assuming that punishing attention is more "hedonic" than neglect, an opponent-process model may be applicable. Contiguity conditioning seems to account for the observation that, e.g., during clinical treatment with a good quality of attention, antisocial behaviour persists for a considerable period of time. These considerations are speculative, but they open up better perspectives than only a single theory.

In conclusion, that there is a basic repertoire of parent-directed behaviour patterns is best accounted for by ethological theories. The subsequent adjustment to the particular circumstances to which an infant is exposed is the domain of learning theory. The weaker attachment if the social figure was exclusively punishing (underemphasized by Rajecki et al.), the adjustment to a parent's conflict strategies, and the stereotyping of substitute parent-directed behaviour in the course of development, can be easier understood from the various learning theories.

That the paper of Rajecki et al. offered these perspectives of an integration of behaviour theories demonstrates that the game of theorizing is worth playing.

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- experience that has been little studied. Our research is an effort to determine whether the devastating effects that some infant macaques suffer when separated involuntarily from their mothers are general among the primates. Results of experimental mother loss are evaluated against detailed records of the langur monkey (*Presbytis entellus*) infant development in normal social contexts. This gives us new insights into the spectrum of potential responses to mother loss in monkeys and suggests how the multiple-caretaker pattern influences the effects of mother loss and potentially increases infant survival.
- Although a langur infant's mother is the most important caretaker for the first few months of life, the infant experiences caretaking by many adults from the day of its birth. This pattern is normal for most Colobines and provides a unique opportunity to compare the effects of differing early experience and developmental consequences.
- Mothers of six- to eight-month-old langur monkey infants were removed from natal social groups for two-week periods. Results include the following:
1. All infants show changes in their behavior during separation. Sensitive behaviors include play, vocalization, and approach and contact behaviors; changes in behavior frequencies range from slight to pronounced.
  2. All infants seek contact and care from females during separation and show strong preferences for certain females. Most importantly, every infant tries, and most succeed, to adopt a female to serve as the major and, usually, only caretaker during its mother's absence. Infants do not seek care from peers, and there is no clear correlation between an infant's choice of substitute caretaker and its early experience with different females. If a caretaker is obtained, symptoms of distress are quickly reduced, but should the infant fail to gain a substitute for its mother, the infant's distress continues unabated for days. These distressed infants usually return to their mothers when the latter are re-united with the group.
  3. All adoptions of substitute caretakers are infant-initiated. Adult females selected by infants may allow adoptions but do not initiate them. Some adult females reject the infant advances and are thus not potential adoptive caretakers. The receptivity of females to infant contact-seeking behavior and attempts at adoption needs to be investigated. Research suggests that both permanent, idiosyncratic aspects of temperament and fluctuating social and physiological factors may influence a female's receptivity (or lack of it) to an infant's approaches. Interest in immature conspecifics varies from female to female, both in the field and in our colony. For as yet unknown reasons, some females invariably show interest in and high tolerance for separation-aged infants, and others consistently reject them.
  4. Following most separations, the infant elects to remain with its adopted caretaker even after its mother is returned. When a mother returns, she does not actively seek her infant; if there is to be a reunion, the infant seeks its mother.
- Evidence from completed separations suggests that two factors are important in determining the amount of distress that an infant experiences during separation: 1) the degree of restrictiveness of the mother, and 2) the availability of substitute caretakers. Our data strongly suggest that the level of distress experience by an infant upon caretaker loss can be manipulated by selecting the infant's social companions.
- Our anticipation that a substitute major caretaker would alleviate the symptoms of distress during separation proved true, but the level of distress was alleviated more by the availability than the adequacy of substitute care. Females selected by infants for adoption were sometimes the most punishing of available adults, and it is possible that simple availability is the crucial factor in adoptions.
- Adoptions extending from separation into postseparation weeks appear to last. No infant returned to its mother after spending weeks with an adopted female. However, additional caretakers may be used for brief periods to supplement time spent on and in proximity with the adopted adult female. Adoptions are also stimulated by weaning and by the birth of an infant to a caretaker. When adoptions occur in these latter conditions, we do not observe the changes in vocalizations, play, or locomotion that occur during separations from the mother.
- The following are important in determining the amount of distress during separations: 1) the mother's restrictiveness, not during the first two months of life, but from the second month till separation, and 2) the availability, not adequacy, of caretakers during separation. Females ranging from harsh to accepting and permissive have been adopted by infants. The adopted female may or may not have an infant, but if she does, it may not deter her from

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**Langur monkey mother loss and adoption.** It is not surprising that current theories of attachment do not permit an understanding of the complex phenomena of infantile attachment in organisms as different as birds, dogs, monkeys, and humans. These animals have diverse life strategies based on a variety of biological, social, and environmental adaptations. Differences between birds and humans, for example, are far more instructive than similarities. In a comparison it is more fruitful to begin with humans and not with other animals; otherwise the comparison is reductionistic and obscures the behaviors that distinguish human beings (Washburn 1978).

Monkeys are too often considered to have a fairly homogeneous set of attachment patterns. In truth, all "monkey" patterns are neither simple nor macaque. The following statement is offered to expand the data base against which theories of attachment may be measured.

Any young infant Old World monkey which has lost its mother faces immediate danger, and during the time between mother loss and the acquisition of substitute maternal care, if such is available, it is in grave jeopardy. The model of infant development common to the well-known Cercopithecines (baboons and macaques) includes the mother as primary if not exclusive caretaker. For them, her loss in the wild means almost certain death to the young infant.

Studies of infant development and the effects of early deprivation have concentrated almost entirely on the macaques. The Colobines, or leaf-eating monkeys, are as numerous and diverse, and their pattern of multiple infant caretakers represents an important variant of infant development and

accepting another. In these instances the adopted infant must compete with others for a place on the ventrum and on a nipple.

All species probably show distress at the loss of an important caretaker. The amount will vary and depends, at least in part, on the availability of substitute caretakers – even if only to the extent that the substitute allows passive contact. No unitary concept of protest/despair in response to mother loss seems appropriate for the langur monkey. Qualities of mothering predispose the infant to effectively seek substitute caretaking, but the availability of such caretaking may be crucial to the degree of separation distress. Infant persistence is usually of no avail if the target female is not receptive. It is likely that there are ranges of reaction to mother loss typical of every species, rather than a single, stereotyped set of responses, and reactions to mother loss presumably depend on a number of factors involving adults as well as the immature. The langur presents a very useful contrast to the macaques. It remains to elucidate the reasons for the differences, but for langur monkeys they will surely include the multiple caretaking pattern, infant initiative, and adult acceptance.

It is apparent that "the social bond" is likely to have differences in attributes, functions, and routes of development in different kinds of animals. To understand the nature of infantile attachment requires looking at the infant, its conspecifics, and the context of social life, which must include normal demands on the animal at all stages of life.

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**Attachment: the two sides of one coin.** Rajecki et al.'s effort to examine the huge literature on attachment from various theoretical viewpoints is undoubtedly a valuable contribution to our understanding of the phenomenon. As a biologist I shall restrict my commentary to the critical issues that the authors have raised with regard to the ethological theory of attachment. Their main criticism seems to be concerned with the observation that children form attachments to inanimate objects or even to sources of maltreatment. According to their view, this observation contradicts the predictions of the ethological theory. As they do not seem to be certain whether this theory can cope with attachment behaviors under conditions of parental abuse (compare statements in the long abstract with the summary under "Maltreatment effects and ethological theory"), I shall not go into this point in great detail. I should only like to emphasize that, in terms of evolutionary theory, the survival value of a specific behavior cannot be assessed in an absolute sense but only in terms of probability.

Concerning attachment to inanimate objects, however, I will suggest three counter-arguments: First, the authors do not consider the fact that the formation of attachments to inanimate objects presupposes the formation of social attachment. Second, we have to be aware of the possibility that the phenomenon of becoming attached to both objects and people simultaneously might be more or less an artifact of western childrearing practices. This view is supported by the cross-cultural study of Hong & Townes (1976), which found that "an infant's attachment to inanimate objects is lower in a culture or social group in which infants receive a greater amount of physical contact, including a higher rate of breast feeding, and in which the mother is more physically involved and available when the infants go to sleep." One of the interesting findings in this study was that "the highest incidence of attachment occurs in the Anglo-Saxon culture, and the least among non-Western groups." Gaddini's (1970) results were similar in her study on three different social groups in Italy (cited in Hong & Townes 1976).

Third, although the evidence from cross-cultural data is still too limited to draw any final conclusions, we must consider the possibility that attachments to inanimate objects are qualitatively different from attachments to social objects, a distinction that merits further experimental investigation. Some evidence for the existence of such differences is provided by the observation that infants may share bonds to inanimate objects, whereas they refuse to do the same with social bonds (Eibl-Eibesfeldt, personal communication). In this context it must be stressed that ethological theory has long

since begun to differentiate among several kinds of attachments in animals (Lamprecht 1973).

Ethologically-oriented researchers have also contributed to resolving the confusion that the superficial biological argumentation originally used by Bowlby (1969) has caused among scientists of other disciplines. It seems that these attempts to improve or correct the theory have escaped Rajecki et al.'s attention. Bowlby's original notions that the major survival value of attachment behavior in infants lies in the potential for better protection against predators, and that the development of bonding is relatively independent of reinforcers such as food and warmth, are no longer accepted as completely valid statements, because they "lack one important step in the zoological argument" (Blurton-Jones 1972). According to this view it is no longer feasible to overlook the fact that man has been semi-carnivorous for the longest part of his evolutionary history, thus occupying a unique ecological niche totally different from his higher primate relatives, who predominantly eat plant food. On the basis of a comparison of the general patterns of mammalian child care, integrating data on milk composition and correlated feeding schedules, Blurton-Jones (1972) stresses the role of nurturing and physical body contact as important contributors to the formation of attachment. Konner (1972) has provided strong cross-cultural evidence for the relevance of this corrective orientation in his study of developmental ethology in the !Kung Bushmen of the Kalahari desert. As a result of his study he also suggests an amplification of Bowlby's statement concerning the functions of attachment by emphasizing that it not only serves to provide protection against predators but also enables the child in this society of hunters and gatherers to learn social interaction patterns and the continuous acquisition of subsistence behaviors from as early as one year of age.

Of special interest with regard to the argument of the article under commentary is Konner's (1972) additional suggestion that it is the combination of innate features of social behavior and reinforcement learning that contribute to the growth of attachment during the first half year. It is a pity that Rajecki et al. fail to discuss the promising possibility of a synthesis between the learning theories of attachment and the ethological model, though they do cite Bowlby's four principles explaining the development of specific and discriminating attachment, which contain similar suggestions.

To be quite explicit: protection against predation cannot be seen as the only selection pressure that has caused the development of attachment behaviors during evolution, because it is not a specifically human selection pressure. The special qualities of attachment formation between the human infant and its mother must also be understood as the result of some other unique achievements in human evolution, like upright gait and encephalization, that led to the specialized "construction" of the human infant. As is well known, Portmann (1969) has stressed the baby's exceptional status compared to the offspring in other orders of mammals by terming it as a "sekundärer Nesthocker" (secondary altriciality). For this reason we totally agree with Rajecki et al.'s notion that species differences must be taken much more into account. Attachment in precocial birds is certainly not directly comparable to attachment in higher primates, or even man, because it developed as an adaption to different environments and ecological niches.

In the last section of this commentary I should like to contribute to the modification of some unfortunate statements made by the authors. The claim "that all of Lorenz' postulates on imprinting can be viewed as incorrect" cannot be accepted, nor that "Hess' ideas on imprinting have not been universally well received." According to Immelmann (1976) and Eibl-Eibesfeldt (1978), there are at least two postulates concerning the phenomenon of imprinting that are still generally accepted as valid: the existence of the critical period as one characteristic component, and the irreversibility of the process once it has occurred. The only qualification Immelmann (1976) makes is that we should rather think in terms of a specific mechanism of information processing and storage than a specific learning device. This last notion induces me to mention that there is increasing evidence of the existence of a "critical period," even for human mothers, which might be regarded as an important constituent in the formation and development of mother/infant bonds (Klaus & Kennell 1976; Hales et al. 1977). In accord with the authors, one should emphasize that we need to get rid of the one-sided view of the attachment phenomenon, which stresses only the child's predisposition toward attachment behaviors. The forces of selection do not act upon only one partner of an interacting system; they act so as to ensure mutual adaptations.



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**Maltreatment effects and learning processes in infantile attachment.** The general strategy employed by Rajecki et al., – namely, to compare the numerous theories of infantile attachment in terms of major, cross-species attachment phenomena – is, I think, an excellent one. However, several portions of their article deserve particular comment. First, there are two peripheral (though, I think, not unimportant) points I wish to make. With regard to Rajecki et al.'s statement that "all of Lorenz' postulates on imprinting can be viewed as incorrect," I would mention: (1) Immelmann's (1972) contention that Lorenz was often referring to sexual rather than simply filial imprinting, and that such characteristics as irreversibility seem to be largely valid for the former; and (2) some of the Lorenzian characteristics (e.g. a critical period) appear to be perfectly valid in a naturalistic context and only break down under unnatural laboratory conditions (Ratner and Hoffman 1974).

Secondly, to say that "Hess' ideas on imprinting have not been universally well received" is no doubt an understatement, as Hess himself would readily admit (Hess 1973 op. cit.). But Hess' "contribution to a general understanding of attachment" must go unquestioned, both in light of the enormous amount of sound empirical data on imprinting that he himself has produced over the past three decades, and – as importantly – in terms of the powerful stimulatory function that his admittedly unpopular views have had in the field as a whole (Eiserer, in press).

Turning to a more central issue, several aspects of Rajecki et al.'s discussion of maltreatment effects also warrant serious questioning. In deriving predictions from the classical conditioning model, for example, the authors rely almost exclusively on the stated assumption that the attachment object serves to reduce a young animal's level of arousal (presumably, arousal of an aversive sort) – even though no such assumption is contained in the model's five basic premises. True, Hoffman himself accepted that assumption elsewhere in his 1973 paper with Ratner, and again in his 1977 paper with DePaulo; but the point is that even if the assumption were to prove false, this would not detract from the essence of the model (i.e., positing a central role for classical conditioning in attachment phenomena).

Furthermore, Rajecki et al. go too far in representing the classical conditioning position as holding that "attachments form (or imprinting takes place) because certain stimulus configurations reduce the subject's arousal" (italics added). While Hoffman has concluded that certain features of an attachment object can (and normally do) reduce arousal upon their first presentation to a newly-hatched duckling, that is not to say that an attachment forms only because of such arousal reduction. Thus Hoffman's position does not preclude the possibility that imprinting could still occur despite an

"abnormal" situation in which the object itself may actually increase the subject's arousal. In short, I believe that Rajecki et al. have erred in making their maltreatment prediction from the classical conditioning model.

Concerning maltreatment effects more generally, the overall conclusion of Rajecki et al. that "we have uncovered no evidence that the maltreatment of infants materially interferes with the formation or persistence of social bonds" seems to conflict with the findings of several studies cited by the authors – namely, (1) Barrett's (1972 op. cit.) and Ratner's (1976 op. cit.) finding that shocked ducklings show diminished preference for shock-associated objects; and (2) the finding by Harlow & Suomi (1970 op. cit.) and Harlow, Harlow, & Suomi (1971 op. cit.) that infant monkeys also show diminished preference for surrogates associated with aversive stimulation. Do diminished preferences fall outside the authors' definition of "interference"?

Again, let me emphasize that Rajecki et al.'s strategy of comparing various theories in terms of cross-species attachment phenomena is extremely worthwhile in principle. However, one phenomenon only briefly mentioned by Rajecki et al., but which I believe must be considered in detail by future analyses of this sort, is the attachment that gradually develops to those features of the surrogate (perhaps its particular color or shape) that are initially neutral in terms of eliciting filial behavior. This phenomenon, in which the neutral features gain behavioral control after being temporally and spatially associated with an innate releaser of filial responses, would seem to represent the very essence of the development of infantile attachments to specific objects. Importantly, the effect has been documented in ducklings (Hoffman, Eiserer, & Singer 1972; Eiserer & Hoffman 1974) and in infant monkeys (Mason, Hill, & Thompson 1971), and could conceivably occur with humans as well (Hoffman & DePaulo 1977 op. cit.).

At least two different interpretations are relevant here. First, the initially neutral features may develop behavioral control through a process of classical conditioning, with the innate releasing feature (e.g. visual motion for ducklings or tactile stimulation for infant monkeys) serving as an unconditioned stimulus in the Pavlovian sense. Alternatively, the innate releaser may merely serve to draw the infant's attention to the surrogate's neutral features, whereupon a process of simple exposure learning occurs until the now-familiar color and shape characteristics can themselves elicit filial behavior.

Various data can be cited in support of the plausibility of both hypotheses, but in a study (Eiserer, unpublished) designed to pit one against the other, newly-hatched ducklings were given pre-exposures to the neutral features of an imprinting object before those features were associated with the innate releaser of visual motion (a second group of ducklings was not given such pre-exposures). A strict classical conditioning prediction would suggest that processes of latent inhibition should retard development of behavioral control by the initially neutral features in the pre-exposed group; the exposure learning hypothesis, however, would predict that the pre-exposures should, if anything, give Group 1 a "headstart" over Group 2 in the attachment process, so that attachment formation in the former should be facilitated. The results indicate that facilitation in the pre-exposed subjects do in fact occur.

Of course, in the final analysis the various processes of exposure learning, classical conditioning, and also instrumental learning will probably all be found to play important – and no doubt complementary – roles in the realm of filial attachment. Teasing apart and delineating those roles offers one of the field's most challenging tasks for the future.

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**Sociobiology, brain maturation, and infantile filial attachment.** A more detailed consideration of the nature of adaptation suggests that Rajecki et al. err in their assertion that infants bond maladaptively, but that they are probably correct in their hypothesis that infants play a more active role in the bonding process than is generally recognized. An active role on the part of the infant is also consistent with current knowledge of the maturation of other forms of behavior and of the brain.

In an evolutionary and sociobiological sense, adaptation consists of the survival of genes over time (Dawkins 1976). Whether or not a given behavior is adaptive can only be judged in terms of its ability to promote the survival of genes by increasing the survival and reproductive success of individual animals or their close (hence, gene-sharing) kin.

According to Rajecki et al., infantile attachment to an abusive mother is maladaptive. The authors fail, however, to consider the alternatives available to an infant with a poor mother. There are only two: survive without a mother, or find another one. For most altricial birds and nursing mammals, the first alternative is impossible, the second nearly so. Adoption has been recorded rarely in wild animals, and in at least one species in which it has been recorded, the chimpanzee, it is usually unsuccessful (Van Lawick-Goodall 1968, 1971). Adoption is, in fact, an improbable event in most mammalian groups, as it requires the synchronous occurrence of an adoptable infant and an infantless, but lactating, mother. Moreover, because adoption usually requires considerable maternal energy expenditures on unrelated or distantly-related kin, in preference to similar expenditures on genetic offspring, it is usually genetically maladaptive for the adoptive mother (Dawkins 1976) and would be expected to occur infrequently in nature, even when circumstantially possible. Given the difficulties of finding an adoptive parent, it is probable that the most adaptive behavior is always for an infant to attach to his own mother, no matter how abusive, and to increase its own attachment attempts if her bonding mechanisms are inadequate. Then, at least, the infant has some chance of survival; otherwise it has none.

According to Rajecki, et al., instances of attachment of captive animals to inanimate objects are further evidence that infantile attachment is often biologically inappropriate. For most animals, adaptation must be considered in reference to conditions encountered in the wild, not in captivity. The ability to distinguish mother from any object imaginative scientists might think to provide for laboratory animals would have no survival value in nature and hence would be unlikely to evolve. Evolutionary adaptation demands only that infants preferentially bond to the mother rather than to unsuitable objects normally present in the environment.

As the biological mother is frequently in close proximity to the infant, all that is necessary to assure adaptive bonding is that infants attach to the familiar in preference to the strange, and that they attach to one or more features always possessed by the mother and not possessed by familiar but inappropriate attachment objects. The attachment of laboratory rhesus monkeys to cloth surrogates (Harlow 1962) suggests that all that is necessary to initiate the attachment process in this species is a soft, clingable surface. This situation would be maladaptive if cloth surrogates abounded in nature, but since they do not, it is actually highly adaptive, in that it virtually assures attachment of the infant to its mother, who always possesses soft, clingable fur. Similarly, birds who attach to styrofoam objects need not be considered to exhibit maladaptive behavior, as it is possible that the attachment in these cases is to features such as color, size, shape, or movement, which the styrofoam object, never present in nature, shares with the mother bird.

While the interpretation that attachments such as those formed by Harlow's monkeys are maladaptive appears erroneous, Rajecki et al. are probably correct in pointing out that they do indicate that the infant plays an active role in forging its own attachments. An active role for the infant is also predictable on the basis of sociobiological theory. An infant who fails to attach to its mother dies; its genes get no second chance for evolutionary survival. In contrast, a mother who loses one infant can usually have others. Since the in-

fant has more at stake genetically than does the mother, it is highly improbable that evolution has provided only mothers with the ability to initiate the bonding process.

Moreover, an active infantile role in the attachment process would be consistent with the mode of development of other behaviors. Neither at the level of motor development nor at the level of perceptual and cognitive maturation can vertebrate infants be considered the passive stimulus-response machines, dependent entirely upon environmental circumstances, that behaviorist theories often imply. In certain species, at least, motor actions precede sensory responsiveness developmentally (Coghill 1929), and active motor behaviors are essential for adequate maturation of both perceptual (E. Gibson 1969) and cognitive mechanisms (Piaget 1952 *op. cit.*). It would be strange if further research should demonstrate that bonding mechanisms alone develop as a result of passive stimulus-response mechanisms.

Finally, neuroanatomical maturation proceeds in a manner consistent with the hypothesis that evolution has not placed animals entirely at the mercy of the environment by providing them only with the ability to passively react to environmental stimulation; rather it has provided vertebrate infants with the distinctly more adaptive ability to determine actively their own behaviors just as at most other nervous system levels, anatomical structures thought to function primarily as motor or output mechanisms mature prior to structures primarily receptive in function (K. Gibson 1970). While the neuroanatomy of the infantile bonding mechanism is not known, two of the areas known to function in adult affiliative behaviors in monkeys — the orbital frontal cortex, and the temporal pole (Kling and Steklis 1976) — follow this same general maturation pattern. In both areas, cortical afferent V and VI layers mature in advance of layer IV (K. Gibson 1970), which receives the majority of cortical afferents, suggesting that active neural mechanisms underlying affiliative behaviors also mature relatively early.

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**The epigenetic character of development.** I appreciate the authors' invitation to comment on their review of infantile attachment. I hope they will forgive me if I take this opportunity to rectify an egregious terminological error that has crept into the theoretical vocabulary of developmental psychology and is not specific to their paper.

Following Moltz (1963 *op. cit.*, 1965), the authors designate T. C. Schneirla's theory (1959, 1965 *op. cit.*) as "epigenetic." This is correct as far as it goes, except that the implications are: (1) that Schneirla's theory is the epigenetic theory, and (2) that there could be a non-epigenetic theory of behavioral development. Both of these implications are incorrect.

As so elegantly detailed by Needham (1959), up until the 19th century there were two antithetical conceptualizations of the process of individual development: preformation and epigenesis. Although there were all varieties of preformation theory, the common assumption was that the organism was fully formed at conception, and that the process of organismic development involved only growth. Epigenetic theories, on the other hand, assumed qualitative transformations during the course of development which, in today's terminology, correspond to the view that individual development is characterized by *differentiation* as well as growth. Due to the direct embryo-

logical observations of Caspar Friedrich Wolff in the 1700's, Karl Ernst von Baer in the early 1800's, and the genuinely experimental embryology introduced by Wilhelm His and Wilhelm Roux, among others, in the late 1800's, the epigenetic character of all individual development was factually documented, and the idea of preformation was discarded.

This momentous conceptual issue now settled for morphological and physiological embryology, it remained only to apply the concept of epigenesis to behavioral embryology:

"In the classical usage of the term, all present-day theories of prenatal behavioral development can be characterized as epigenetic. This term denotes the fact that patterns of activity and sensitivity are not immediately evident in the initial stages of embryonic development and that the various capabilities of the organism become manifest only during the course of development. However, major disagreement exists with regard to the fundamental character of the epigenesis of behavior. One viewpoint holds that behavioral epigenesis is predetermined by invariant organic factors of growth and differentiation (particularly neural maturation), and the other main viewpoint holds that the sequence and outcome of prenatal behavior is probabilistically determined by the critical operation of various endogenous and exogenous stimulative events." (Gottlieb 1970, p. 111)

Since the above was written, a number of epigenetic theories or models of behavioral development have appeared, and their common theme is the transformational character of behavioral or psychological change, not the processes or mechanisms posited to account for these epigenetic changes. In other words, epigenetic theories are not monistic; there are a variety of epigenetic points of view (see, e.g., Cairns 1978; Gottlieb 1973, 1976; Kuo 1976; Waddington 1968; Werner 1948).

In conclusion, then, insofar as the six "earlier peripheral theories" and the five "current direct theories" alluded to by Rajecki et al. in their review are developmental theories, they are all also epigenetic theories. Schneirla's approach/withdrawal theory is only one of many epigenetic theories of "attachment."

I apologize to the authors for highlighting an issue peripheral to their main concern, but the present forum seemed to provide an especially propitious occasion to comment on a serious terminological misapprehension in developmental psychobiology.

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**How can we test attachment theories if our subjects aren't attached?** Attachment presumes at least three criteria: 1) the ability of the young to discriminate and respond differentially to the object of attachment, 2) preference for the attachment figure and differential proximity-seeking, and 3) response to removal of the attachment object, which is distinct from responses to the reduction of social stimuli per se (Ainsworth 1972 *op. cit.*). Differential responding, in addition to social interaction between mother and young, is central to the attachment concept. The attachment figure is singled out from other similar social stimuli to play a special, unique role in the infant's environment (Bowlby 1969 *op. cit.*). In studying attachment in human infants, we have applied the above criteria fairly stringently (Cohen, 1974). In

using various animal models to test aspects of attachment theories, we have been much more lax. Like others in the field, Rajecki et al. make the assumption that the young of most species which are dependent on maternal care for survival do form attachments. This is not the case, and it is time that we take note of this fact in using comparative data to elucidate the attachment process.

The need to differentiate between species that do and those that do not form attachments was recently demonstrated by Mendoza et al. (in press). They contrasted the consequences of separation of mother and infant in the rat with the consequences of separation in the squirrel monkey. In both species mother-infant interaction is complex, with the behavior of both mother and infant being regulated by a variety of subtle cues (Rosenblum 1968; Smotherman, Mendoza, & Levine 1977). However, in the rat there is no evidence for attachment. Rat pups do show a response to the removal of the mother. Even so, these responses appear to be a consequence of physical events occurring upon separation rather than a response to the mother's absence. When the physical consequences of the mother's removal are prevented, responses to separation are not shown (Hofer 1975). Furthermore, the rat mother does not show a response to the removal of her offspring (Smotherman et al. 1977). The data also indicate that neither recognition nor preference play a primary role in maternal behavior of the rat (Moltz 1971; Rosenblatt 1965).

In contrast, both the mother and the infant squirrel monkey demonstrate a specific mutual attachment. Squirrel monkey infants have been found to be capable of maternal recognition when given a choice between the mother and either a familiar (Rosenblum 1968) or unfamiliar (Kaplan & Schusterman 1972) adult female. In addition, both the mother and infant show a response to separation independent of the presence of other social stimuli. Although the infant may be mothered by another familiar female during its mother's absence, this does not reduce levels of physiological arousal, which have been found to be as high as those shown when the infant is separated from the mother and kept in isolation for the period of separation (Coe et al. 1978).

If we apply the criteria of specialness, or uniqueness of the attachment figure to the species used in Rajecki et al.'s review, two points become evident. First, there is no evidence for attachment in the studies reviewed on dogs. And second, in many of the other studies discussed there was no evidence that the responses described were specific to the social stimulus designated as the object of attachment. The young of a number of breeds of dogs do show distress upon separation from mother and littermates (Elliot & Scott 1961 *op. cit.*; Pettijohn et al. 1977 *op. cit.*). But this distress is not specific to the absence of the mother. Elliott and Scott (1961 *op. cit.*) showed that following a period of being alone, distress vocalizations were reduced to the same extent whether the puppy was presented with the mother or with an unfamiliar female of the same breed. Furthermore, neither the data on "secure base" effects or maltreatment effects provides evidence for the existence of attachment bonds in dogs. As Rajecki et al. noted, the use of unfamiliar conspecifics in the studies on social facilitation of exploration in dogs means that these studies cannot be used as tests of secure base effects. And it should be noted that in none of the studies of maltreatment effects was it demonstrated that the prosocial responses shown by the puppies were specific to the particular human adult delivering the abusive treatment.

Similarly we can question whether the data on precocial birds demonstrates the formation of attachment bonds between cage- or brood-mates. Again, the data do clearly indicate an increase in distress calls upon separation from members of the brood. But, Gaioni et al. (1977, *op. cit.*) showed that replacing brood-mates with unfamiliar conspecifics, such that the number of social partners remained the same, prevented such responses. They concluded that the young ducklings used in their study were not "attached" to the other members of the brood but were responding merely to the reduction in social stimuli.

There are a number of reasons to differentiate between responses to social stimuli and attachment. First and foremost, the keystone of the attachment concept is that the attachment bond is specific. Any theory of attachment must in some way account for the specificity of the relationship (Smith 1969). To test the adequacy of attachment theories, using the young of species which do not form attachment bonds, or using social stimuli with which the infant has not formed an attachment relationship, can only add confusion to an area already fraught with controversy. Second, social responses in the context of attachment relationships can be viewed as forming a subset of social responses in general. But factors which affect responses to the attach-



ment figure may or may not have equivalent effects on social responses to other social stimuli. In fact, in order to determine the etiology of attachment, it will be necessary to identify those factors that have an exclusive or primary effect on the infant's responses to its object of attachment. Finally, we do not know which factors characterize species that form attachment bonds, and which differentiate these species from those that do not form such bonds. Identification of these factors would certainly enrich our understanding of the attachment phenomenon.

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## by Eckhard H. Hess

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*The road to general attachment theory: little headway.* I want first to make some comments concerning omissions and cavalier dismissals of the work of contributors to the area generally encompassed by the terms "infantile attachment" or "social bond." Clearly, the pioneering work of Gilbert Gottlieb, who was one of the first to study the social bonding of precocial birds in nature, should have been cited. Similarly, Klaus Immelmann, whose work on sexual imprinting in altricial birds could probably serve as a better model for a comparative approach to social bonding in mammals, is not even mentioned. But perhaps most startling to me is the reference to Konrad Lorenz's contributions to imprinting. After listing an abbreviated version of Lorenz's statement, Rajecki and his collaborators conclude that "the provocative notions [italics mine] stimulated an enormous amount of research, the result of which is that all of Lorenz's postulates on imprinting can be viewed as incorrect." What is really incorrect is this statement by Rajecki et al. (a statement that has been similarly used so many times by P. P. G. Bateson).

The fact is that there are a goodly number of studies, by serious investigators, and the introduction of *Imprinting* (Hess & Petrovich 1977) deals specifically with this distortion of Lorenz's contribution. Some of this distortion is evident also in Rajecki et al.'s paragraphs dealing with my own work. The reader who is unfamiliar with the literature on imprinting would conclude that I have spent my years making a "claim that there is a critical period for imprinting in precocial birds," and have done not much else. I will not respond to these statements, but I assume that the interested reader can refer to my original publications.

Having unburdened myself of what appeared to be an immediately bothersome aspect of Rajecki et al.'s paper, let me now address the major thesis, which is the supposed substance of the paper and its potential contribution to science. Early in the text the authors state that "From these dual perspectives, empirical and theoretical, the aim of the current paper is relatively straightforward. We now have a fair idea what the phenomenon of attachment looks like, and we know what various theories of attachment look

like. It would seem a reasonable matter to collect the data and the theories in one place, and to assess the fit between the two. This is what we have attempted to do." Certainly this goal can only be described as reasonable and laudable. But there are little hints throughout the course of the paper that the outcome is a foregone conclusion and really something that we knew all the time, given, of course, that the reader has a reasonable familiarity with both the data and the current theoretical approaches that seek to account for those data. So far, to me at least, Rajecki et al.'s review was an opportunity to be reminded of familiar studies and familiar theories, and, with the exception of Gewirtz's learning theory, where I felt the authors to be not quite fair, nor on the same wavelength as my own reading of his material, it should serve a useful purpose for general readers to obtain a quick overview of the various theoretical positions in regard to attachment.

The selection of data to use as a countercomparison is, however, another matter. For example, a page is used to give the evidence for "maltreatment effects in birds." The studies quoted all more or less lead to the same conclusion - that is, in precocial birds procedures involving pain do not reliably prevent the formation of some degree of attachment. From Kovach & Hess (1963 *op. cit.*) to Rajecki et al. the story is pretty much the same. It would perhaps have been preferable to amplify data sections dealing with maltreatment effects, secure base effects, and separation effects in children. At least from my observations it would seem that while there are often seductive similarities between social behaviors in some animals when compared to humans, the bottom line on our supposed goal regarding human behavior is that it is precisely human behavior that we are trying to understand. Of course, the larger evolutionary picture may be worthwhile looking at, but by not the slightest stretch of the imagination would I expect to find infant social bonding to follow the same general rule in, say, a mallard and a cowbird. Rajecki et al. touch on this when they say that they "were unable to demonstrate in chicks the multiple peer separation effect (maturational arrest) that has been documented in infant and juvenile monkeys." Probably this last section of their paper deserves greater emphasis and elaboration for any proposed *general* theory of infantile attachment.

My own considered opinion is that Rajecki et al. do not take us far down the road "Toward a General Theory of Infantile Attachment."

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## by Howard S. Hoffman

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*On the matter of interpretation and judgement in the evaluation of theory.* Any effort to juxtapose theory and data in the domain of infantile social attachment must, of necessity, be a highly selective procedure. The topic is simply too broad, and its literature is too complex and too scattered to be concisely summarized. In this document Rajecki et al. have elected to focus on the infant's response to involuntary separation from its attachment object, on the role of aversive stimulation in the formation of this attachment, and on the influence that the object exerts over the infant's responses to other objects and events (e.g. the so-called secure base effect). In doing so, they have performed the useful function of calling attention to three of the more salient aspects of social attachment, and they have touched on a good many of the theoretical issues that its analysis entails. They have not, however, resolved those issues (at least not to this commentator's satisfaction), nor have they provided much in the way of valid insight into directions that future efforts should take.

Perhaps the most obvious problem is that in elaborating a good portion of the theoretical material, Rajecki et al. have made distinctions and drawn inferences that seem better described as personal interpretations than as disinterested accounts. For example, opponent process theory is, at least by implication, cast into opposition to classical conditioning theory, and each is separately called to task for its failure to properly account for one or another aspect of the data on attachment. Opponent process theory is criticized for standing mute on secure base effects, while classical conditioning theory is faulted for its failure to account for separation effects.

As an investigator who has helped promulgate both theories, I find this strategy quite misleading. While the classical conditioning theory was designed to elaborate how a subject learns the features of a given attach-

ment object, the opponent process theory was intended to explain how the presence and absence of the object (and its features) control various motivational and emotional aspects of the subject's behavior. To criticize classical conditioning theory for failing to account for separation effects, and to fault opponent process theory for not explaining secure base effects, is like criticizing a theory of color vision because it fails to deal with size constancy, and faulting a theory of size constancy because it says nothing about color vision. Clearly, no single explanatory system is likely to account for all aspects of visual perception, and it would be unreasonable to expect anything different in the domain of social attachment.

An even more serious problem is that Rajecki et al. provide ample evidence that they have failed to grasp certain of the most basic aspects of the theories that they are seeking to evaluate. For example, their discussion of classical conditioning theory repeatedly refers to the *formation* of social attachments, yet this theory has never asserted that such attachments form. Rather, the theory describes a learning process whereby an object that *innately* elicits a filial response can be rendered familiar – thereby preventing it from being novel and hence generating fear when the subject is older. As indicated in several of the papers that deal with this theory, and as implied by its five premises (which Rajecki et al. present here), the theory holds that imprinting is not so much a procedure for cementing a social bond as it is a way of protecting an innate reaction tendency from future interference by a competing response.

A similar failure to grasp essential features of the material under scrutiny is seen in Rajecki et al.'s discussion of Ratner's (1976 *op. cit.*) investigation of the effects of aversive stimulation on filial behavior. In that study Ratner found that after receiving electrical shock in the presence of an imprinting stimulus, ducklings preferred a somewhat novel stimulus when they were subsequently offered a choice between it and their original imprinting stimulus. This result runs counter to most of the other "maltreatment effects" that Rajecki et al. describe, and in commenting on it they suggest that it may have been an example of a preference for "slightly novel stimulation" (Bateson 1973 *op. cit.*), and that the shock treatment may have been quite irrelevant. It is puzzling that the authors should offer this suggestion, since they had already noted (indeed in the same paragraph) that Ratner's control subjects (e.g. ducklings that were never shocked) showed no such preference, a finding that would indicate to most investigators that the occurrence of shock was crucial to the effect.

Equally misleading is Rajecki et al.'s report of Ratner's additional finding that when offered a choice between their original stimulus and one that was quite different, ducklings that had received shock during exposure to the imprinting stimulus exhibited an overwhelming preference for that stimulus. What Rajecki et al. failed to report is that this effect was only obtained in the initial test with these subjects, and that with continued testing, the birds came to exhibit an overwhelming preference for the novel stimulus. In short, as is the case with Rajecki et al.'s treatment of theory, their interpretation and judgement of data appears also to have been strongly influenced by their personal views.

Perhaps it is too much to expect a bias-free account in an enterprise of this sort. Certainly the amount and diversity of material to be covered was quite prodigious, hence some measure of interpretation and judgement was required if any progress was to be made at all. But in this commentator's view the need for interpretation and judgement should have demanded a much deeper grasp of the issues than this document seems to reveal.

Fortunately, it is the nature of science that even highly complex topics, such as those treated here, come eventually to yield some of their secrets if investigated carefully and persistently enough. In the meantime, the present effort might, if viewed with a clear head, provide a useful catalogue of those complexities, even if it fails to resolve them.

#### by Klaus Immelmann

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**Imprinting and infantile attachment.** It is certainly a good idea to compare attachment theories, to point to their strong points and possible weaknesses and to try to develop a general theory of infantile attachment. However, I have some specific comments to make regarding imprinting.

One of the six "peripheral theories" Rajecki et al. review at the beginning is Lorenz's paper on imprinting. I would like to stress two points in this connection. First, it is not quite clear to me why this "theory" is included in a list

of theories of attachment, because there seems to be no primary correlation between imprinting and attachment at all. In filial imprinting of young precocial birds, to which the authors refer, two aspects have to be kept clearly separate: there is an (obviously genetically coded, i.e. "innate") tendency to follow a moving object which, under natural conditions, is always the mother. This behavior is called the following response. Knowledge of the object to which this response is directed, in contrast, is not at all coded genetically but has to be acquired through the first, or a subsequent following response in the bird's early life. And it is this acquisition process which has been called imprinting, in the original sense that Lorenz defined the phenomenon. In other words, the tendency to form an attachment is there without previous experience – i.e. without any imprinting – and the latter just determines which object is selected, thus creating the necessary prerequisites for the formation of filial or sexual attachments with one particular individual or class of individuals.

In sexual imprinting, for example, which in many ways provides a much better example of the phenomenon of imprinting in general (Immelmann 1972b), the relevant preferences are established very early, at a time when the young individual is still dependent on its parents, whereas definite attachments are not formed until weeks or months later when pair formation actually takes place. The same applies to other forms of imprinting such as locality, habitat, or host imprinting. It follows that filial imprinting – i.e. the rapid formation of a social preference leading to the almost immediate occurrence of attachment – is just one type of imprinting. If one refers to imprinting in general, however, the whole range of phenomena listed under this name should be taken into account. And here I can see no immediate relevance to attachment, especially to human infantile attachment. Furthermore, the general applicability of "imprinting theory" to the formation of attachments is even more restricted by the fact that, as is well known to ethologists, many attachments are formed without any imprinting taking place at all.

Second, the authors claim that they estimate "the current standing or status of each of the positions." With respect to imprinting, however, they have failed to do so. The three characteristics they cite to be typical of imprinting (which give neither complete nor entirely correct credit to Lorenz's original statements) have long since been discussed and more or less replaced by a broader characterization of the phenomenon of imprinting. Furthermore, the statement that "all of Lorenz's postulates can be viewed as incorrect," is simply incorrect. On the contrary, in several studies it has been shown that all of the characteristics of imprinting mentioned by Lorenz are indeed realized (cf. Schein 1963, Schutz 1965, Immelmann 1972a). A review of the current status of the imprinting position, and of the many misinterpretations of early papers has been provided by Immelmann (1972b) and by Immelmann & Suomi (1978).

With regard to the review of Hess' critical period concept of imprinting, I agree with the authors' view that there certainly is no genetic mechanism that "strictly (and strongly) limits critical periods for imprinting." Such limits are indeed weakly delimited and, to a large extent, depend on environmental and experimental conditions. However, this finding does not preclude genetically-determined constraints on the occurrence of sensitive phases of imprinting processes. Comparative studies have indeed revealed species differences in the duration of sensitive phases that can be attributed to different ecological adaptations (Schutz 1970, Klopfer 1959, 1964, Immelmann 1972b).

The main point of my commentary is that I am surprised at the authors' surprise that the effect of maltreatment on the maintenance and even on the formation of attachment should be comparatively small. From the evolutionary point of view, this is to be expected. Under natural conditions the "object" to which the first following responses of young precocial birds are directed is always the natural mother. She guides and calls the young and, of course, never "maltreats" them. It follows that maltreatment by the imprinting object is a situation that never occurs under natural conditions. Thus there has been no necessity and thus no selection pressure to develop a specific reaction to it.

I think the rather small effects of maltreatment just provide additional evidence for the strength of the following response and the tendency to form and maintain an attachment to a "mother object." This is strong enough to lead even to some degree of attachment to an object by which the young individual has been maltreated (if no other object for attachment is available).

The same applies to the nature of the imprinting objects as discussed by

the authors, who characterize the ethologist as proposing that since the bonding process represents a biological disposition, it is *most likely* to occur under biologically appropriate conditions; but "instead," they add, it also occurs with respect to quite unnatural objects. I can see no contradiction here, and I cannot understand the word "instead." There is a large body of evidence (for a review, see Immelmann 1972b) that imprinting is achieved most easily toward a natural object – i.e. a living conspecific – and that it becomes more and more difficult to achieve, and leads to less and less exclusive and permanent bondings, the more unnatural the imprinting object is. If two objects are offered simultaneously, the more natural one is chosen to form an attachment to. So, imprinting is indeed "most likely" to occur with a biological object, and the fact that (if no natural object is available) bonds can also be formed with less natural objects is just another indication of the strength of the tendency to form an attachment under any circumstances.

Finally, I would like to refer to one general point of Rajecki et al.'s paper. In their conclusions and recommendations the authors state that before any cross-species generalizations can be made, one has to take "serious account of species differences." Being a biologist, and being aware of the variety of species-specific adaptations to particular environments, I really couldn't agree more. In their paper, however, the authors do not seem to follow their own recommendation particularly seriously, because again and again it is stated that such and such is the case in "birds, monkeys, and humans," "birds, monkeys, and children," or "nonhuman primates."

Most of the bird data to which the authors refer are based on studies with chickens. The domestic chicken is a precocial species, and we now know that, with regard to the mechanisms of attachment formation and maintenance, profound differences exist between precocial and altricial birds (Klinghammer 1967). We also know that differences are also to be found *within* precocial birds (e.g. between ground-nesting and hole-nesting species, Klopfer 1959). In addition, we know that during the course of domestication, behavioral deviations do occur (Sossinka 1971), so that data on domesticated strains of the chicken do not necessarily permit any final conclusions on the relevant mechanisms operating in their wild ancestors. Conclusions based on data on the domestic chicken, therefore, should be drawn very carefully.

If the authors had followed their own advice, they probably could have given a more detailed picture of the attachment story in animals, which in turn might have made the common principles (e.g. possible phase specificity of development, necessity, and nature of attachments) and their adaptive character even more apparent.

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**Evolution, interaction, and object relationship.** I wish that Rajecki et al. had taken seriously in the body of their paper three points they make in clos-

ing: (1) that there is danger in "the notion that identical bonding processes need to be involved" in all species; (2) "that infants may be impelled to form attachments;" and (3) that infant *emotions* are importantly involved in "the phenomenon of early infantile attachment."

In the course of evolution a series of successful adaptations – the change from external fertilization to internal fertilization by copulation, the development of the amniote egg, then the placenta and viviparity, and then mammary glands and milk – have led to increasingly greater viability of offspring. What also evolved was improved parental care – a system of feeding and protection for the more slowly-developing infant that has led to increased closeness, a shared experience, and a more durable bond between mother and infant (Kaufman 1970). In view of the increasing importance of the mother (in the course of evolution) to the survival of the slower growing, more functionally dependent infant, it is understandable that natural selection has favored the emergence of processes which bind the infant to the mother (and vice versa). In early life these are automatic biobehavioral processes (e.g. clinging, nursing, crying vocalization), but then through repeated and varied life-serving interactions, *psychological* processes develop (in higher forms with the requisite neural equipment). These include strong feelings toward the mother and the wish to remain with or near her, because of the meaning the mother has to the infant and its emerging way of life, so that her absence produces anxiety and then longing and depression (Kaufman 1977).

Not displaying such an evolutionary perspective, the authors fail to distinguish between ultimate and proximate causes of behavior, and they seek proximate processes of attachment common to all species from birds to man. Yet, Hinde (1961) has pointed out that birds and mammals, or their reptilian ancestors, have been distinct since the Carboniferous; they have different neural equipment; parental care has evolved independently in the two groups; and similarities in behavior are not likely to be based on similar underlying (i.e., proximate) mechanisms. Further, when we consider the psychological aspects of attachment, we enter a domain of behavior in which man may be compared meaningfully only to other primates.

It is these psychological aspects, so critical in primates, that are strangely lacking in the authors' purview, thereby probably explaining why they overlook or minimize a number of significant studies and ideas about attachment and object relationship. Schneirla (1965, *op. cit.*), for example, did more than point out the role of stimulus intensity in approach-withdrawal behavior. He emphasized the importance of reciprocal stimulation (i.e. interaction) in bonding, and described the evolutionary progression from biotaxic to psychotaxic processes (directed orientations based on psychological factors), and from biosocial to psychosocial bonds, in which the *meanings* rather than the immediate physiological effects of stimuli are functional (Tobach & Schneirla 1969).

Psychoanalysis is also dismissed with a shopworn, simplistic, outdated, and incorrect rendering of its theory as gleaned from one clinical paper by one author. Overlooked are studies (e.g. by Erikson (1950), Fraiberg (1977), Freedman (1971), Mahler et al. (1975), Sander (1974), and Spitz (1965)) that document an epigenetic development of psychological processes, including attachment and object relationship, in which maturation of specific sensory and motor apparatuses provides the leading edge for certain kinds of encounters with the environment and *interactions with the mother*. The "dialogue" with the mother, as Spitz (1965) aptly called it, is the critical experience out of which the relationship develops and gets specified. The amount of interaction is important, but so is the nature, especially the appropriateness and contingency, of the mother's responses.

It is surprising that the authors have failed to note that interaction with the mother is the basis of attachment in every theory they examine except possibly opponent-process. The theories differ principally only in the manner in which the interaction operates to produce attachment. (The so-called "ethological" theory does differ importantly from the others in the breadth of its perspective, but not with respect to the basis of attachment.)

Infants are impelled by their evolutionary history to behave in species-typical ways that, given species-typical experience, will lead to a relationship with the mother, the type and durability of which have also been selected and have a species-typical range (Kaufman 1975). The relationship emerges from the interaction with the caretaker, but its specific nature depends on the specificities and complexity of the neural equipment and the rate, order, and extent of its maturation and development, which in turn depend on the appropriateness, contingency, and regularity of the caretaker's responses. At the behavioral level, neural equipment translates



to processes and functions such as conditioning, learning, memory, and concept formation, the pattern of which determines the nature of the attachment and the object relationship. In higher primates, and especially in man, the object conserved is endowed with rich attributes, personal meaning, and strong feelings. That is why in higher primates it is more useful to speak of object relationship than of attachment, the latter summoning an image of a static, simple state of being joined. Perhaps if the authors *thought* of object relationship, instead of attachment, they would better appreciate the rich, dynamic, psychological processes involved.

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by Joseph K. Kovach

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**Infantile attachment: a general theory or a set of loosely-knit paradigms?** By embarking on a comparative evaluation of the theories of infantile attachment, Rajecki et al. have entered a major arena in the study of behavior. That the early experiences of the child may influence the thoughts, feelings, and actions of the adult has been known since antiquity. The more recent history of psychology is replete with the theme of early experience, and with related skirmishes over the nature-nurture controversy. An evaluation of the type undertaken by Rajecki et al. is essential for the ultimate resolution of the latter, and it has been long overdue. This alone would make the authors' effort a valuable contribution. But there are other reasons as well for commendation. Rajecki et al. have managed to distill a set of paradigmatic predictions and to zero in on data that reveal a variety of conceptual shortcomings in the field. Their conclusion – that there is no general theory or model which would explain and predict all that is already known about infantile attachment – is well documented and is most readily accepted by this commentator. I shall concentrate here on what I have come to regard as the crux of the matter in this contribution: the question of whether or not a general theory of infantile attachment is possible or desirable.

**What is infantile attachment?** Attachment is customarily regarded as a developmental process that includes (a) the formation of the initial bond between mother and infant, (b) the bond's transformation into the progressively broadening social ties and interactions of the growing organism, and (c) its influences on the sexual and parental behaviors and attachments of the adult. The first of these arbitrarily delineated steps includes the infant's attachment to the mother on an equal footing with the mother's attachment to the infant; it depends on the reciprocity of the behaviors of mother and infant. The second step starts long before the first is over; it cannot be pinned to narrow stages and sequences in behavioral development or defined by anything but overlapping response categories. Finally, the expression of parental behavior in the third step feeds back on the first one.

Consequently, a general theory of infantile attachment cannot escape dealing with the entire process, which includes the formation of the initial bond, its transformation to broader social ties, and its influences on adult behavior.

When viewed in this broader context some of the theories and studies considered peripheral by Rajecki et al. become central. I have in mind, especially, the discovery of imprinting by Lorenz, Schneirla's epigenetic interpretation of behavioral development, Scott's studies of early socialization, and the general search for the psychodynamic roots of human personality in mother-infant interactions.

Contemporary concern with early attachment arose from observations of imprinting in birds by ethologists (Whitman 1919, Lorenz 1935, Fabricius 1951, Hess 1959) and from alarming reports that "maternal deprivation" of institutionalized children may have devastating consequences for their psychological and physical survival (Spitz 1945, Bowlby 1951 *op. cit.*). Particularly relevant was the observation by Whitman (1919) that hand-raised doves and pigeons exhibit sexual responses to the caretaker's hand, and that such a habit may become so strongly structured that it interferes with the bird's normal sexual behavior. From this followed Lorenz's (1935) observation of early imprinting and his proposition that it is this quick learning in the neonate that determines both species identification and the adequacy of stimuli for eliciting social and sexual responses in the adult.

Since these early studies, an increasing tide of interpretations, hypotheses, and postulates has been devoted to early attachment, usually with an interest in its long-term effects on behavioral development. Thus, for example, in addition to the postulates examined by Rajecki et al., Bowlby (1969 *op. cit.*) also holds that human attachment takes place during a critical period (the first year of the child's life), that the infant attaches himself most strongly to one person (a phenomenon specified by the term *monotropy*), that the first bond differs from later attachments to other persons, and that early mother-infant interaction has far-reaching consequences for the emotional and social adjustment and well-being of the child and adult.

Whether or not the attachment formed in early life is stronger, more lasting, and qualitatively different from attachments formed at later stages, it is abundantly clear that in all higher vertebrates the infant's survival is predicated on a strong bond with a parent or caretaker, and that this bond exerts a strong influence on subsequent social behaviors.

Studies of attachment in animals (Klinghammer & Hess 1964; Shutz 1965) and man (Schaffer 1963; Rutter 1972; Tizard & Tizard 1974) indicate that the formation of early bonds extends over longer periods than previously believed, that the hypothesis of monotropy is very probably incorrect, that the social experiences intervening between infancy and maturity are important in influencing adult attachments, that multiple attachments may occur in particular settings, and that the strength and pattern of attachment behaviors are characterized by a great deal of individual and species variations. While it may depend on genetically determined capacities to perceive stimuli that the neonate has never encountered before (Kovach 1978), and on responses that are not learned (Hess 1973 *op. cit.*), the formation of a particular bond between a given neonate and parent or surrogate is a matter of associations and learning wherein the object of attachment is a primary dispenser of reinforcements (Bateson 1974 *op. cit.*; Hoffman and Ratner 1973 *op. cit.*). Early attachment emerges from these and many other studies (for reviews see Bateson 1966 *op. cit.*; Schneirla 1964 *op. cit.*), not as an isolated affair but more like a link or series of links in a moving chain that are lead into view by links that precede and slide away imperceptibly with those that follow.

**Is a general theory of infantile attachment possible?** An attempt at adding together all the above so as to form the empirical content of a single general theory of infantile attachment would have to resolve the issues of homology or analogy in the mechanisms identified by species comparisons. It would also have to deal with the immensely complicating issues of cognition and culture in human attachment. I suspect that the resulting theory would quickly grow into a metatheory of the type best characterized by the psychoanalytic and socioanalytic interpretations of Freud and Marx – it could neither claim nor be subject to verification by the paradigm testing procedures of science.

Recent examinations (Kuhn 1962; Popper 1974) have demonstrated, I think quite compellingly, that the effectiveness of a scientific inquiry is proportional to its structuredness by circumspect paradigm-theories. Testing such theories requires collecting (a) facts that they indicate to be particularly revealing, (b) facts that support or negate their predictions, and (c) facts that resolve their ambiguities. Compatibilities among such theories are de-

manded only to the degree of demonstrable overlap in their predictive claims, which must be related to unambiguous continuities in their logical and empirical contents. Accordingly, competition among paradigm-theories is a matter of contradicting prediction and disparate interpretation of observations that they jointly claim to be revealing or ambiguity resolving.

Rajecki, et al.'s search for points of contact between theories and facts on infantile attachment fits this picture only in part. It does not fit entirely, (a) because it brings together and compares diverse interpretations, each of which claims a set of different observations as particularly revealing; (b) because (with the exception of Solomon's opponent-process theory, which, however, is not a theory of attachment *per se*) these interpretations are not circumspect enough for unambiguously identifying what is and what is not shared in their logical and empirical contents; and, therefore, (c) they cannot be refuted by lack of contact with facts that may or may not belong to their claimed contents.

The works of Waddington (1961) and Bertalanffy (1969) suggest that there are a great many diversities and redundancies in the mechanisms that ensure a particular developmental accomplishment in a living system, and that these may be proportional to the survival value of the accomplishment. Accordingly, data indicating that, in the absence of more adequate stimuli, neonates attach themselves to inanimate and punishing objects, or that this learning is unusually resistant to extinction, or that some of its mechanisms do not agree with known processes of social learning, may imply no more than the likelihood that evolution and natural selection ensured a great deal of diversity and redundancy in the mechanisms of early attachment, without which the survival of a young bird or mammal would be unlikely. Such data need not negate the role of biological adequacy and reciprocity of stimulation under natural conditions of attachment, or the importance of positive reinforcement, or the pertinence of conditioning, or the involvement of affective and motivational dynamics.

While the evidence of early attachment is overwhelming, there appears to be no existing or potential avenue that would lead to a single scientific theory accounting for all its manifestations, ramifications, and complexities. What is needed now, for getting on with the job of understanding the apparently diverse and redundant processes of early attachment, is not the construction of a new general theory, nor the pitting of existing theories against each other, but the formulation of circumspect paradigms, each with its own content and procedures for empirical working through, each seeking compatibilities with others through overlapping predictions and against the backdrop of the continuities in what is known or suspected about the evolutionary-ontogenetic determination and neurobiological mediation of this highly complex behavior.

**Conclusions.** These, then, are the conclusions I drew from the paper by Rajecki et al.: *A general theory of infantile attachment is neither possible nor desirable at the present stage of our knowledge. Instead, further efforts are needed to transform existing theories into circumspect paradigms that would account for the various processes of early attachment and would specify areas of unambiguous overlap and interface in empirical contents, prediction claims, and observations.*

As a final note, I wish to emphasize that these conclusions do not negate or diminish the value of the paper under scrutiny. Rajecki et al. have tested a set of circumspect interpretations of infantile attachment with an abundance of pertinent data. Their approach of trying to establish contact between predictions and facts is certainly sound. This is the way science works. Yet, I hope that the conclusions I have drawn from their contribution will urge them on to follow it up with a closer examination of the boundaries and shared territories in the logical and empirical content and claims of the theories they have examined.

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**Implicit assumptions regarding the singularity of attachment: a note on the validity and heuristic value of a mega-construct.** One functional advantage of open peer commentary is that it brings some of us to the point of saying things that otherwise, under other conditions of print and peer review, may lurk in the mind, be discussed at seminars, but never reach the printed page. I would like to take advantage of the present commentary to make such a statement which it relates to the literature review and theory evaluation provided by Rajecki et al.

While George Miller (1956) may have been haunted by a magical number 7 ( $\pm 2$ ), my personal spectre has to do with attachment, and it is a very magical but uninspired number: 1 ( $\pm 0$ ). First of all, let us acknowledge that talking about attachment is like talking about motherhood and apple pie – or, rather, just about motherhood – in that any discussion of the concept, particularly when there are differences of opinion ("knowledge") between the discussants, is likely to be quite heated and generally affective in tone. It is easy to agree about attachment but quite difficult to disagree. Furthermore, let me propose a second law of attachment: again, like motherhood and apple pie, differences of opinion may prevail regarding the precise nature of the concept or processes by which it operates, but the most heinous crime of all is to even consider that the concept itself is faulty, either in substance or usage.

And this is precisely the problem that I wish to address in this commentary. Attachment is. The attachment process is important to understand. Infants form a social bond to their (primary) caretakers. It is uniformly and unilaterally assumed that a singular (although perhaps internally complex) construct of attachment is psychological reality. Even though individual theories may wax and wane in the professional eye and undergo forced evolution to keep pace with accrued empirical results, and even in a paper such as the one by Rajecki et al., which demonstrates convincingly that no single theory, no matter how complex, can adequately or compellingly comprehend empirical findings in three behavioral domains, one parameter that continues to escape objective scrutiny is the underlying assumption that a singular concept is under study. The conclusions and recommendations begin by asserting "... the attachment process" and end with another singular normalization, "... the phenomenon of early infantile attachment. ..."

In psychology, as in other sciences, it has long been standard practice to utilize theory in advance of empirical work as a heuristic tool, defining critical problems and allowing scientific inquiry to proceed with a reduction (not elimination, mind you) of trial-and-error, and without the need to do every-possible-experiment before inducing from systematic covariation and interdigitation of findings the more molar (theoretical) constructs that give psychological meaning and establish consistency. While this procedure may generally be an effective modus operandi, I would argue that it need not always be such. Specifically, in the present instance I would argue that the empirical data and descriptive systematizations (e.g. "secure base effects")

are growing complex in ways that are not compatible with the continued assumption, implicit or otherwise, that attachment is a unitary phenomenon for which a single conceptual label has heuristic value (cf. Masters & Wellman 1974).

In our science it is important to realize that the mere fact that there is a verbal label for a psychological construct does not imply some naive wisdom regarding the singularity or internally consistent nature of the denoted conceptual domain. We have been reminded of this before, as when Hull warned against the perils of reification (Hull 1943), and the time is ripe for another reminder. This does not mean, of course, that a label such as attachment no longer serves a useful summarizing purpose for the grouping of information and speculation about selected classes of behavior, cognition, and affect, and their determinants. It does mean, however, that an unwarranted (or not yet warranted) inductive leap is contained in thinking and discussion that denotes a singular social bond, affective tie, or other psychological entity relating two individuals.

In short, there is one conclusion that Rajecki et al. have omitted from their review. And it is this: the systematization of "attachment" data appears to be most consistent and heuristic on a descriptive-behavioral level and not on the level of an overall mega-construct of attachment. It is meaningful, then, to talk of secure base effects or maltreatment effects on limited subsets of behavior, perhaps even of separation effects (although the conditions of separation are so varied that even this behavioral/descriptive category is possibly multidimensional); evaluating the adequacy of molar theories of learning through the ethological context to account for observed effects, within and across species, is still a meaningful endeavor. But the time has come (in fact, has passed) I think, for us to shift our mental gears and discontinue the psychological search for the secrets of the attachment process, the social bond, or the attachment between one individual and another. Let us now concentrate on the data base and the emerging or apparent categories of social/motor, cognitive, and affective behavior that develop in an articulated fashion according to ordinarily expectable contexts and categories of experience, and/or genetic species-general or species-specific factors that pose the assumed constraints and determinants of behavior in general. And mother, I still love you.

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**Attachment theories maltreated?** This paper provides a useful current review of comparative research on three different attachment phenomena that seem to pose varying degrees of problems for most theories of attachment. The reader is left with the impression that no current theory of attachment is viable, although Bowlby's and Ainsworth's ethological theories probably fare the best. We have two major kinds of objections to the conclusions reached in this paper. First, we have found several instances in which there are inconsistencies in the way certain studies are used to criticize some theories and not others. Second, we have found numerous places in which claims are made that a given theory cannot explain, say, maltreatment effects, and yet a deeper understanding of the theory in question reveals that it can. Throughout, we have found places where there has been a failure to utilize relevant data that, although not originally referenced by the theorists themselves, would most likely be used by the theorists if they were asked to explain the phenomena in question.

As a first instance of inconsistency, Hoffman's theory is criticized for failing to explain why enhanced attachment does not always follow aversive stimulation. Yet ethological theory, which also predicts enhanced attachment, is not criticized on the same grounds. Second, when the suggestion is made that Gewirtz's theory should be totally rejected, this is again based on an inconsistency in the analysis of maltreatment effects. Although maltreatment is argued to enhance attachment sometimes, and thus support

Bowlby's theory, in discussing Gewirtz's theory the authors state that the "social bond is essentially independent of negative reinforcement or punishment." Furthermore, this inconsistent conclusion is then used as a premise for arguing that the formation of social bonds is not dependent on positive reinforcement in the first place, a complete nonsequitur.

Third, and again with regard to maltreatment effects, there is no consistent distinction made between studies in which a) the source of the aversive stimulation is the attachment object itself, versus something else; b) the attachment object is always (from the beginning) punitive, versus only occasionally; and c) the effects of maltreatment are measured during versus after the aversive stimulation. These distinctions are made in discussing ethological theory, and the conclusion is drawn that ethological theory cannot easily explain situations in which the source of maltreatment is the attachment object itself, or in which maltreatment occurs from the beginning. However, in the criticism of Gewirtz's operant theory, such distinctions are not maintained.

This in turn leads us to our second kind of criticism – that the present analysis of some attachment theories fails to appreciate their complexity or the full extent of the foundations on which they are based. For example, a learning theorist such as Gewirtz could explain maltreatment effects if the maltreatment is inconsistent, because partial reinforcement and occasional punishment are known to lead to extreme persistence of the behavior in question and to frustration/punishment tolerance (e.g. Amsel 1967; Brown & Wagner 1964). Gewirtz's theory could also explain maltreatment effects that do not start at the beginning of attachment, i.e., where there is a previous history of positive reinforcement. Like ethological theory, then, Gewirtz's theory has trouble primarily when the maltreatment is consistent and occurs at initial exposure to the attachment object. Interestingly, a review of the cited literature reveals no instance of a study in which substantial attachment occurs when the attachment object never emits any positive reinforcement at anytime.

A second example of the failure to appreciate the complexity and richness of some theories' ability to explain certain data occurs in the treatment of opponent-process theory. First, the presentation of the theory itself reveals a basic failure to understand Solomon & Corbit's (1974 op. cit.) distinction between the underlying a- and b-processes, and the resultant hedonic/behavioral states (A and B), which are the algebraic summation of these processes. For example, the b-process is produced as a result of the a-process (not the A-state as maintained in the paper), and it is the growth of the underlying slave b-process (not the B-state as maintained in the paper) which produces the adaptation-like effects in the A-state. Furthermore, the paper maintains that opponent-process theory cannot predict qualitative changes over the course of a given B-state. We submit that this is not the case. The theory predicts both quantitative and qualitative changes. For example, withdrawal from an addicting drug, a primary example of a B-state, has a number of qualitatively different phases. Analogously, monkeys separated from an attachment object often show a biphasic protest-despair response upon initial separation. Furthermore, three primary examples of opponent-process phenomena used by Solomon and Corbit (drug addiction, the behavior of dogs after repeated sessions of electric shocks, and the emotions experienced by parachute jumpers following the jump) all clearly demonstrate that qualitative changes in the B-state can also occur over repeated stimulations (craving → withdrawal; stealthy and hesitant → joyful; stony-faced → euphoric). In monkeys, too, the protest-despair reaction changes both qualitatively and quantitatively over repeated separations, with protest dropping out and despair becoming more intense (Suomi, Mineka, & Delizio 1978 op. cit.).

It is also argued that opponent-process theory cannot account for maltreatment effects, because maltreatment would produce a negative A-state that should subtract from the positive A-state induced by the attachment object, resulting in zero attachment behavior during the punishment. This is, in fact, what often happens. However, Rajecki et al. fail to note that the after-effect of the maltreatment will be a positive B-state that should increase as a function of the number of maltreatments. This positive B-state could easily account for increased attachment behaviors following maltreatment, with the attachment object in fact becoming a CS for the positive B-state. This could occur even if the attachment object were the source of the maltreatment, as long as maltreatment does not occur all the time.

Furthermore, although the authors imply that opponent-process theory cannot account for secure base effects, we claim that it can. If an attachment



object induces a positive A-state, then the negative A-state induced by any frightening stimulus presented in the presence of that attachment object should be substantially reduced in intensity. Solomon (1978) has, in fact, recently presented data consistent with this prediction. Results from his laboratory show that the duration of tonic immobility in chicks is influenced by the presence or absence of an imprinting object (secure base). Specifically, the duration of the immobility reaction is greatly reduced if immobility is induced in the presence of an imprinting stimulus, and it is substantially lengthened if induction occurs after removal of the imprinting object (separation distress – a negative B-state – compounds the fear underlying the immobility reaction). Furthermore, the extent of the modulation of the immobility reaction is a perfect function of what stage of the A or B state the chick is in. Overall, then, we maintain that a fuller understanding of opponent-process theory negates many of the criticisms presented here.

One last example of a failure to appreciate the complexities of, in this case, ethological theory, will serve to amplify our criticism. Ethological theory is found to be weak, since it would not predict that organisms would become attached to unresponsive objects that bear little resemblance to biological beings, and/or under conditions of maltreatment that fall outside the limits of “an ordinarily expectable environment.” These criticisms fail to appreciate that ethologists have made extensive use of artificial stimuli in their study of the control of instinctive behavior. In fact, the whole concept of sign stimulus implies that artificial stimuli may be at least as effective as real stimuli in eliciting a given set of behaviors. In addition, the artificial stimuli used in the study of attachment are generally *not* unresponsive – e.g., surrogate monkeys provide contact comfort – and imprinting stimuli move. In fact, without these “responsive” qualities attachment does not generally occur.

With regard to the normal limits of maltreatment, it should be remembered that attachment objects under *normal* circumstances often dispense aversive stimulation (e.g. Hinde 1974). Furthermore, that attachments occur in abnormal environments under conditions of maltreatment is not so surprising if one considers that in most such studies there are no alternatives. If there is an alternative object present, preference for that object emerges (e.g. Barrett 1972 *op. cit.*). And if no alternative object is present, who is to say, from an evolutionary standpoint, whether there would have been more selective pressure for an infant to stay close to its abusive mother or to be left completely alone with no one to ever protect it?

In summary, this paper has accomplished one of the purposes which a good review must – the presentation of relevant literature – and, in this regard, the paper has much to commend it. However, the second purpose of a good review – to criticize the theories proposed to account for the empirical work – has not been as successfully fulfilled. We agree that each of the theories has its faults, but, as discussed above, we do not feel that the paper has been entirely fair or consistent in its criticism. At times it seems that an *a priori* assumption has been made that all the theories were wrong (to lesser or greater degrees!), and then a paper has been written that supports just such a belief. And finally, the third and self-professed goal of a review article does not appear to have been attained at all: we do not appear to have moved any closer “Toward a General Theory of Infantile Attachment.” If it is in fact true that all theories have serious short-comings, then more clear-cut suggestions as to how these theories might be integrated would be useful. For example, ethological theory may best explain how and why attachment occurs in the first place, classical and operant learning theory may help us understand many of the important factors in the maintenance of attachment, and opponent-process theory may provide us with a model to look at temporal changes in the dynamics of attachment and separation over time (e.g. see Mineka & Suomi 1978 *op. cit.*). We believe it unlikely that any one theory will ever fit all the data. Rather, an advancement of understanding of attachment phenomena will probably require the integration of several theories which can be seen as complementing one another rather than being incompatible with one another. Future review work should address itself more directly toward such a goal.

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**Learning theory and infantile attachment: a re-evaluation.** There has been a long need in developmental psychology for a carefully prepared article that reviews and unifies human and subhuman research relevant to infants' filial attachments. Rajecki et al. are highly successful in this endeavor. These authors discuss and integrate many significant issues rarely addressed in the attachment literature. However, in the difficult task of comparing and appraising current attachment theories, their article is less satisfactory. Rajecki et al. examine the failings and successes of selected theories with respect to children's reactions to their attachment figure during maltreatment, child-initiated separations (secure base effects), and forced separations. On this basis they reject sizable portions of each of the diverse theories, but especially the learning analyses of attachment representative of Cairns, Gewirtz, and Hoffman. Yet, the potential utility of the learning approaches may have been underestimated, perhaps because of an inadequate regard for their complexity. In some respects, these theories were first presented rather superficially and then faulted because they did not sufficiently account for the particular phenomena being examined.

According to Gewirtz (1972 *op. cit.*; 1976; Gewirtz & Boyd 1976), attachment is a concept referring to those of the child's response patterns controlled by various discriminative and reinforcing stimuli emitted by the attachment figure. Although not acknowledged by Rajecki et al., these attachment stimuli, like other stimuli, are subject to principles basic to learning theory, such as stimulus generalization and generalized reinforcement (Bijou & Baer 1965). Thus, contrary to the suggestions, the attachment figure, from the learning perspective, is more than just “a source of positive primary or secondary reinforcers.” Due to stimulus generalization, the child's behavior may also be controlled by similar stimuli, as when color films (Passman & Erck, in press), monochromatic videotapes, and audiotape recordings (Adams & Passman 1978) of the attachment figure engender secure base effects. Even inanimate objects such as security blankets and pacifiers appear to emit cues that control the child's behavior and result in secure base effects (Halonen & Passman 1978; Passman 1977 *op. cit.*; Passman & Weisberg 1975 *op. cit.*). Because attachment figures (including cloth surrogates and blankets) have presumably been discriminative for a wide variety of reinforcers (tactile stimulation, warmth, food, sleep, etc.), they may come to function as generalized reinforcers (Bijou & Baer 1965; Passman & Weisberg 1975 *op. cit.*). Therefore, extinction or satiation along one relevant stimulus dimension would be expected to disrupt the utility of, for example, a lactating wire surrogate or a food-dispensing, key-pecking device (cf. Harlow & Zimmerman 1959 *op. cit.*; Marley & Morse 1966 *op. cit.*) far more seriously than a generalized reinforcer (cf. Gewirtz 1967).

One of the major arguments against the operant view was that attachments appear to form even when the attachment figure is the source of maltreatment. It was reasoned that if the attachment figure exclusively dispensed aversive stimulation but attachments nevertheless developed, reinforcement could not be a causative factor. However, to rule out the possibility that animals and children receive positive reinforcers from an abusing social object is not as simple a matter as was implied. The abusive source may be rewarding because of other, nonaversive responses it makes (feeding, hugging, playing, etc.) or merely its physical characteristics (warmth, tactile stimulation, etc.). For instance, touch may be a sufficiently potent reinforcer for approach behaviors of primates, even (and, perhaps, especially) when other aspects of the attachment object are associated with pain (cf. Harlow & Harlow 1971 *op. cit.*; Rosenblum & Harlow 1963 *op. cit.*). This possibility is particularly compelling in light of finding that children react differentially to textures (Pack & Weisberg 1976) as well as to adults with different styles of interaction (Weisberg 1975). Moreover, as Cairns (1972) has noted, clinging and proximity to an abusive primate may actually inhibit attack and thus be adaptive (i.e. reinforced). In sum, children would be expected to evidence attachment behaviors to an abusive source if the positively-reinforcing contingencies outweighed the aversive.

Similar criticisms were made against the classical conditioning analysis. In Hoffman and Ratner's (1973 *op. cit.*) theory, specific features of attachment objects can reduce arousal while others are neutral. Rajecki et al. interpreted this model to assume that maltreatment induced arousal which should consequently disrupt prosocial responding to the abusive figure. Therefore, when an attachment figure is the locus of aversive events, it should no longer reduce arousal. However, clinging and other forms of tactual stimuli can reduce arousal (Mason 1968); if this tactile function is innate (Hoffman & Ratner 1973 *op. cit.*), it should not be lost during maltreatment, so long as the attachment figure is available to provide tactile stimulation. This point can be investigated empirically by subjecting young animals to inescapable noxious stimulation from their attachment object. Measures of heightened arousal should be lower when clinging is allowed than when it is not. A related problem for Rajecki et al. is that the initially neutral features of the abusive attachment figure acquire positive rather than aversive properties. As they note, this question cannot be answered easily. Nevertheless, it should be noted that the pairing of stimuli with events ostensibly both positive and aversive is not unique to the imprinting and attachment literature. The fact that associations form to one class of events more readily than to another does not eliminate learning as a useful explanation. Instead, the need for a more detailed analysis of events is indicated, with attention given to various possible constraints on cue utilization (Bolles 1973; Seligman 1970).

Child-initiated separations (exploration), despite contentions by Rajecki et al. to the contrary, can be derived from operant theory. Because of a variety of possible situational factors (such as satiation or extinction), the attachment figure's discriminative and reinforcing control may be temporarily weakened, and the child may approach other, competing reinforcers in the environment. In addition, Hoffman & Ratner's (1973 *op. cit.*) model proposes that initially-neutral features of attachment figures can gradually come to evoke approach and arousal reduction through associations with attachment stimuli that intrinsically elicit these behaviors. This concept helps explain early phases in infants' developmental shift from proximal to distal styles of contacting attachment objects (Passman & Weisberg 1975 *op. cit.*; Walters & Parke 1965). Because auditory and visual aspects of the attachment figure are consistently paired with tactile stimuli that elicit the filial behaviors, they should acquire distress-reducing properties via classical conditioning. As a result, these distal stimuli begin to take on the attachment functions of the proximal stimuli, and separations may be promoted through distal contact alone (Adams & Passman 1978; Passman & Erck, in press).

With regard to involuntary separations from the attachment figure, repeated experiences progressively diminish the magnitude of protest among infrahumans (e.g. Elliot & Scott 1961 *op. cit.*; Suomi, Harlow, & Domek 1970 *op. cit.*). Rajecki et al. argue that the operant position would predict an increase, not a decrease, in these reactions after multiple separations. An infant's protests upon separation would ordinarily be reinforced (according to some schedule) by the return of the attachment figure. However, under conditions of continued, forced separation, protest responses would be extinguished because they are ineffective in producing a return of the desired figure. The resultant decrement in protest following repeated separations, and the substitution of other operant responses (such as "despair"—i.e. learned helplessness—Seligman 1975), are consistent with the research cited by Rajecki et al.

As Rajecki et al. conclude, each of the theories they have evaluated possess components that do not completely fit the current data. However, we believe that a sufficient amount of research does not exist at this time to make final, conclusive judgments about the overall utility of any of these theories. The explication of interactions between learning processes and a variety of species-specific tendencies is a challenge to all attachment theorists. At present it seems that a combination of approaches is necessary for an adequate interpretation and understanding of attachment phenomena.

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## by Michael Rutter

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**Attachment: its meaning and consequences.** The attachment process necessarily involves social learning, and as such it will be influenced by the variety of factors that modify any form of learning. The evidence confirms that this is indeed the case. However, as Rajecki et al. emphasize, the more interesting and important question is whether there is anything about the process of developing attachments that makes it different from other forms of learning. This most thoughtful and scholarly review admirably brings out the challenges to the leading theories posed by the findings on the consequences of maltreatment and of separation, and by the secure base effect. Their conclusions on these phenomena are both balanced and well-substantiated. However, there are further issues which also need to be considered in any adequate theory of attachment.

**Varieties of attachment.** To begin with, there is the question of how far attachment may be considered as a unitary behaviour (Coates, Anderson, & Hartup 1972; Rosenthal 1973; Stayton & Ainsworth 1973). The empirical evidence indicates that it is *not*, and that at least two distinctions need to be made (Rutter 1978a). First, there is the difference between the general tendency to seek attachments and the formation of selective bonds that are personal, social, and reciprocal. The importance of this distinction is evident both from monkey and human studies. For example, the early attachments to inanimate objects shown by Harlow's monkeys did not lead to normal social relationships as parent or peer attachments usually do (Harlow & Harlow 1969; Ruppenthal, Arling, Harlow, Sackett, & Suomi, 1976). Also, young children reared in institutions show *more* clinging and following than family-reared children, but nevertheless they are less likely to show selective bonding or deep relationships (Tizard & Rees, 1975). The findings may mean that the processes involved are different, or, more likely, they may indicate that the nature of the attached object's *response* to the infant will influence the



quality of the relationship formed and hence its function in relation to later development.

The second distinction is between secure and insecure bonding (Stayton & Ainsworth 1973). It has often been assumed that amount of distress on separation is an indicator of the strength of attachment (Schaffer 1971), but it has been found that this measure does not necessarily agree with other indices of attachment. Thus, strong attachment (as shown by greeting on reunion together with following behaviour) may be accompanied by less crying on separation (Stayton & Ainsworth 1973). It seems that the distress probably reflects insecurity of relationships as well as the presence of attachment. It is also pertinent that Hinde & Spencer-Booth (1970) showed that rhesus monkeys' distress after separation was strongly associated with prior maternal rejection and tension in the infant-mother relationship. It is evident that the quality of attachments, as well as their strength, must be taken into account. Again, the nature of the reciprocal interactions with the infant is probably crucial in this connection.

*Attachment and other social relationships.* The problem of whether the concept of attachment encompasses all positive social interactions in young children involves two rather separate issues (Rutter 1978b). First, there is the question of whether all positive relationships constitute greater or lesser degrees of the same phenomenon of attachment. The evidence suggests that they do not. A crucial distinction appears between social play that is inhibited by anxiety, and attachment that is intensified. Thus, Lamb (1977) found that when a child was with his parents, the entrance of a stranger reduced playful interactions but increased attachment behavior. Children may prefer to play with peers (Eckerman, Whatley, & Katz 1975) or even a stranger (Ross & Goldman 1977) but will nevertheless prefer to go to a parent for comfort. The same applies to rhesus monkeys (Patterson, Bonvillian, Reynolds, & Maccoby 1975). Play and attachment overlap greatly, but they exhibit rather different qualities (Heathers 1955; Ross & Goldman 1977; Harlow & Harlow 1972 *op. cit.*) and seem to serve somewhat different purposes (Hartup 1978), except in the unusual circumstance of rearing in the absence of parents (Freud & Dann 1951; Ruppenthal et al. 1976).

The second point is whether early bonds between parent and child underlie or form the basis of later social relationships. There is surprisingly little evidence on the extent to which this is so, but two recent studies (Tizard 1977; Tizard & Hodges 1978; Dixon 1978) of children reared in institutions with multiple caretakers are relevant. Continuities were found between excessive clinging and more diffuse attachments in infancy, attention-seeking, and indiscriminate friendliness at four years, and impaired relationships with adults and other children in middle childhood. It appears that early bonding is linked with later socialization. It is also important that Tizard & Hodges (1978) found that children who were later adopted (and experienced a good stable upbringing) showed the same social and attentional problems in school as those who had remained in institutions. Attachments can develop after infancy, but it may be that fully-normal social development is nevertheless dependent on early bonding.

*Role of the infant.* Rajecki et al. note the need to find out more about the infant's role in the attachment process. It is known that attachment still occurs in infants with such gross physical handicaps as blindness or lack of limbs (see Schaffer 1971), but it may be that the characteristics of the infant play a greater part in relation to timing. It is necessary to account for the observations that attachments do not usually develop until after at least three months of age, and that there is considerable individual variation in when this occurs. There is a lack of satisfactory evidence on whether there is an age beyond which it is too late for normal social bonding to occur for the first time (Rutter 1978a & b). However, the indications are that there may be something of a sensitive period for this process, even though it is not sharply defined and is influenced by experimental factors.

*Conclusions.* Rajecki et al. argue that any adequate theory must account for the existence of the phenomenon of attachment, for the fact that it may occur with inanimate objects, for the observation that it develops in spite of various adverse experiences, and for the finding that it serves a number of specific functions. This is certainly true, but it is not enough. There must also be an explanation of the timing of the attachment process, of its qualities and varieties, and of its long-term course and developmental sequelae. These further considerations underline many of the points made in Rajecki et al.'s review, but they also suggest that greater weight may need to be given to the role of the child and to the influence of social learning in shaping the qualities and course of attachment.

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## by Eric A. Salzen

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*Orientation and affect in infantile attachment.* The Bowlby-Ainsworth ethological theory of attachment reviewed by Rajecki et al. is closely comparable with a neuronal modelling theory of imprinting which I have described (Salzen 1962, 1968 [in Salzen 1978] 1970 *op. cit.*). More recently (Salzen 1978) I have reviewed social-attachment data (primarily for non-human primates and man) and attempted to detail a complete attachment theory in terms of an orientation mechanism for sensory homeostasis and a theory of emotion which can account for the affective elements of attachment. The present review by Rajecki et al. uses some of the same data, together with data for nonprimate forms, and identifies certain difficulties in the Bowlby-Ainsworth theory of attachment. The most constructive commentary that I can make, therefore, is to indicate how my extended ethological theory can resolve such difficulties.

*Maltreatment effects.* My attachment theory distinguishes between distance stimulation, which elicits and guides approach and withdrawal as a



unitary orientation response, and contact stimulation, which terminates and reinforces this orientation response. As in Bowlby's theory, the preferred or optimal configurations for both types of stimulation are partly evolutionarily determined and partly determined by the individual's experience both *pre* and *post-natally* (neuronal or perceptual modelling). Discrepant stimulation leads to orientation movements that restore sensory or perceptual homeostasis. Maltreatment invariably involves discrepant contact stimulation and must enhance orientation behaviour. This will be directed to the source of maltreatment if this is also the most appropriate or the only distance stimulus source. Alternative sources will be approached and become preferred if they provide some appropriate contact stimulation. This is recognized by Rajecki et al. in their footnote 5. However, they do not note the fact that maltreatment invariably involves intermittent and partially-appropriate contact stimulation. Thus, even the learning theories of attachment could account for enhanced orientation to the abusive parent. But enhanced attachment-orientation behaviour must not be confused with enhanced attachment. In my theory the strength of attachment means the degree of definition or specificity of the perceptual model of the attachment figure, and this is determined by preference tests. Attachment behaviour consists both of orientation movements and signalling behaviour, occurring when the orientation tendencies are thwarted or in conflict. Thus, maltreatment will both enhance and thwart orientation behaviour, and will hence lead to signalling such as vocalizations (including pleasure calls upon intermittent contact). If maltreatment is strong enough, it may reduce, change, or even stop orientation movements; but this should lead to other thwarting responses such as aggression and neurotic inactivity. In the absence of alternative goals, perceptual modelling should still take a place, and this will be revealed in preferential orientation and signalling. All these predictions are in accord with the data reviewed by Rajecki et al.

**Secure base effects.** My attachment theory proposes that the infant orients in gradient fields of perceptual discrepancy. Slight unfamiliarity elicits forward movements (approach), while greater discrepancy brings in a turning component that produces withdrawal. A strange figure will elicit turning and retreat to the focus of concordant perception (the secure base), but as adaptation or perceptual modelling of the intruding figure occurs, so discrepancy will fall to a level that elicits approach (exploration). The affective accompaniments parallel this continuum of discrepant perception, with fear at the high end and pleasure at the low (when orientation behaviour is ending and adequate contact is being achieved). This mechanism fits Bowlby's treatment of secure base behaviour, which Rajecki et al. criticize because biologically-inappropriate objects can become secure bases. This mechanism allows for such objects operating in the absence of biologically more appropriate ones which function as better nodes of sensory/perceptual concordance.

**Separation effects.** In both Bowlby's and my own attachment theory, separation responses are orientation-behaviour-directed to previously-established perceptual goals. Where substitute objects are available, these goals may be sufficiently matched and so transfer will take place. This is the case with the infant rhesus in the study of Mason & Kenney (1974 *op. cit.*), where dogs provided comparable animate distance and contact stimulation. Without substitutes, orientation behaviour is chronically thwarted, and, according to the emotion theory outlined in my review paper, thwarting responses such as neurotic inactivity (despair) should occur. Multiple separations should precipitate despair (repeated thwarting), but multiple transfers will lead to generalized perceptual models and easy transfer – i.e. to no "strong attachment." Concordant evidence is reviewed in my paper and is evident in the present review by Rajecki et al.

**Conclusions.** In their conclusions Rajecki et al. note three deficiencies in attachment theories which my own theory of orientation and affect in infantile attachment may be able to meet. The theory attempts to specify the necessary and sufficient conditions for attachment (distance stimulation for eliciting, and contact stimulation for reinforcing orientation behaviour). It allows for species differences in both the appropriate stimulus requirements and specific orientation behaviour. It accounts for the effect of security associated with the attachment figure (pleasure from stimulus concordance and its associated declining orientation behaviour, and distress from stimulus discrepancy and its accompanying orientation and thwarting responses). Thus I have tried to show how objections to the Bowlby-Ainsworth ethological attachment theory can be met by the extended theory of attachment and emotion which I have described elsewhere. Nevertheless,

in their present review of attachment data and theories, Rajecki et al. have indeed identified some of the outstanding difficulties with which any new theoretical development will have to deal.

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**The systems theoretic approach to social behavior.** This is a commendable effort to integrate a complex and basically important field of research. It is particularly important because the authors have adopted a truly comparative approach, including birds, monkey, dogs, and humans. From this viewpoint the only shortcoming of the article is the failure to include an even wider variety of species, including the non-precocial birds, ungulates such as sheep and goats, and rodents such as guinea pigs.

It would be easy to write a commentary as long as the paper itself. I shall therefore confine my remarks to the portions of the paper that directly concern the work of my colleagues and myself: experimental work on attachment and separation in dogs, critical periods, and my own theoretical approach to the problem of attachment. To begin with the last, my theoretical approach (described in the paper as a learning theory) is systemic-organizational-developmental-evolutionary. As such, it is inherently inclusive rather than exclusive.

From the standpoint of systems theory, the concept of attachment is a metaphorical one based on mechanistic theory. I have therefore stressed the concept of *primary socialization* to indicate that the basic phenomenon is the initial step in the formation of social relationships involving mutual interaction, rather than the action of external forces on a passive object. From the view-point of organizational theory, a social relationship involves two-way interaction, a tendency toward increasing stability of organization, and along with this, almost inevitable appearance of a critical period or periods.

Developmentally it is possible to infer the nature of the organizational processes involved. Rajecki et al. apparently missed the restatement of my assessment of such processes (Scott, et al. 1974); briefly stated, all that is necessary for attachment to take place is some form of perceptual contact. The simplest possible hypothesis that will account for the results is that the animal must develop two capacities: 1) the ability to discriminate between familiar and unfamiliar objects, which is obviously dependent upon the organization of sensory capacities; and 2) the capacity for memory (familiarity implies that an animal can recognize the object seen before). The simplest form of memory is associative learning, and repeated exposure to an object should render associations stronger. Still another capacity involved in attachment is the development of the distress reaction in response to separation from a familiar object. Once this response is organized, it should serve as a negative reinforcer, punishing separation. Learning processes should thus strengthen attachment in two different ways but do not themselves comprise the whole process, which also includes physiological processes of growth and differentiation. To the extent that learning is involved, the above theory is essentially similar to that of Cairns (1966a *op. cit.*).

Rajecki et al. have rejected the negative reinforcement portion of this theory on the ground that separation distress appears at the time of first separation. On the contrary, the theory states that separation distress *must* appear at the first separation after this capacity is developed. It is not separation distress that is learned, but the motivation for effecting a reunion, in accordance with the laws of reinforcement. To my mind the only direct test of this hypothesis was made by Compton (1972). The results were ambiguous, perhaps because of the techniques and small numbers employed. Nevertheless, puppies that had experienced frequent unalleviated separation distress showed greater average dependency (measured by time spent nursing) and proximity to an alleviating stimulus (self image in a mirror).

Finally, from an evolutionary viewpoint the similarities among attachment processes in such widely different forms as birds, canids, primates, and ungulates are so striking that they can hardly be accounted for on the basis of convergent evolution. A universal phenomenon among vertebrates (even fishes) is that of site attachment. I have suggested that this is a basic process common to all vertebrates, and that it has been secondarily adapted for social attachment in those animals that show the latter phenomenon.

Further, the process of primary socialization should evolve in relation to the type of social organization characteristic of any given species. For example, the timing of the critical period for socialization in dogs results in strong attachments being formed to other members of the litter, thus forming the basis for later pack organization, whereas the critical period in humans is so placed that it ordinarily produces the strongest attachments to parents or caretakers rather than peers, being correlated with the characteristic multi-generational organization of human social groups.

Rajecki et al. have dismissed the topic of critical periods in a somewhat cavalier fashion. There are two aspects of critical periods. One is an objective phenomenon: that social attachments occur more readily in certain periods of development than others. This phenomenon is so widespread and so well established that it cannot be doubted. The only questions at issue are the rigidity and timing of critical periods. The theory of critical periods, on the other hand, belongs to general organizational theory and is too involved and complex to discuss here, (Scott 1978a, 1978b).

My final comments concern the interpretation and reporting of research on dogs, which has been done almost entirely in our laboratories. All evidence leads to the conclusion that the process of attachment takes place very rapidly during a critical period from approximately three to twelve weeks of age and is not dependent on external rewards or punishments. Attachment will take place at later periods in life, but at a much slower rate. Ordinarily, social attachment is a mutual process, and we would predict that any two individuals that spend on appreciable amount of time together and are aware of each other will become mutually attached, resulting in the formation of a dyadic social system. The quality and complexity of the relationship will be modified by external factors and the nature of the interactions between the members of the system, but the basic process is unaffected. This finding has important and far-reaching implications for both dogs and humans.

Consequently, it should not be surprising that Fisher's (1955 *op. cit.*) punished puppies did not avoid the persons to whom they were exposed. Indeed, after punishment had been discontinued, these puppies were more attentive to humans than those that had been always indulged.

With respect to secure base effects, Pettijohn et al. (1977 *op. cit.*) compared the effect of the mother and an unfamiliar adult on separation distress and obtained no difference, probably because both mother and strange adult were disturbed by the strange situation and paid little attention to the puppy. Here again, the concept of the "secure base" is a mechanistic one. We are actually dealing with a social relationship, and the behavior of the infant, whether it be canine or human, will largely depend on the behavior of the adult companion and the interaction between the two. We do know, developmentally, that litters of puppies reared in a large one-acre field did not begin to explore the environment more than twenty feet or so away from their kennel until approximately twelve weeks of age (Scott and Fuller 1965).

In regard to separation, Rajecki et al. have failed to mention the most important separation phenomenon in dogs, the kennel dog or separation syndrome. Dogs that have been reared in a restricted environment and then suddenly removed from it at an age of six months or older exhibit a behavioral syndrome characterized by extreme fear of strange places, events, and humans. These responses can be reduced by training but nevertheless persist over periods of years. The syndrome is rapidly eliminated by restoring the animal to the home kennel (Scott et al. 1973 *op. cit.*).

The separation syndrome is not, strictly speaking, infantile behavior, but it is the result of infantile attachment. Symptoms of the syndrome begin to appear, in less drastic forms, shortly after twelve weeks of age. Defining infancy in terms of dependency, dogs are almost completely dependent on others for survival until approximately 16 weeks of age.

In addition, Rajecki et al. have misinterpreted the results of repeated periods of short separation. If puppies are separated for a ten-minute period once a week, there is essentially no change in the rate of vocalization until approximately nine weeks of age, when the rate begins to drift downward, probably because of maturational changes. Inexperienced puppies vocalize at rates only slightly higher than those that have been previously exposed (Elliot & Scott 1961 *op. cit.*).

If the puppies are separated briefly once a day, the rate of distress vocalization declines, but never below a level of approximately 50% (Scott, Stewart, & DeGhett 1974, 1977). Presumably, if the puppy were separated often enough, it would become familiar with and attached to whatever was in the new environment.

In conclusion, I agree with the authors that the theories presented by them

and criticized in their article are not completely satisfactory and are certainly incomplete. To my mind, the principal shortcoming of these theories is that they attempt to explain the origin of a social system on a basis of mechanistic theory. While such theories can be included within a systems theory, and may be quite adequate to explain limited portions of a system, they can never be complete. The Bowlby-Ainsworth theory, which the authors have labeled ethological, comes the closest to being a systems theory and, indeed, seems to have the most general significance.

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*Infantile attachment and exposure learning.* I have no doubt at all that Rajecki et al.'s survey and appraisal of infantile social bonding is timely and very worthwhile. Nothing as comprehensive, and nothing that scrutinizes the accumulated data in relation to the predictive capabilities of the main existing theories, has appeared on this topic heretofore. I admire the business-like implementation of the avowed aim of the article. Perhaps not surprisingly, I find myself in almost total agreement with the analysis presented and the conclusions reached by the authors. I shall therefore confine myself to the following: (1) some general reflections on the theoretical situation in the field in question, (2) one specific (rather marginal) observation, (3) some comparative data on guinea pigs, and (4) some glosses on the phraseology and conclusions.

The multiplicity of seemingly incompatible theories of infantile attachment has long been a source of vexation, as well as a challenge to research workers and "consumers of research" in this area. A very useful review and evaluation of the state of a section of the area - viz. imprinting - was published not many years ago by one of the authors, Rajecki (1973 *op. cit.*). Later I attempted to provide an overview of the theoretical state of affairs with regard to imprinting (Sluckin 1975), and although I did not accept one of Rajecki's interpretations, I did agree with his conclusions. These conclusions, however, appear to be somewhat at variance with those of the present article by Rajecki et al. Rajecki (1973 *op. cit.*) considered the neuronal model (e.g. Salzen 1970 *op. cit.*) as the most promising approach to the explanation of imprinting. I tried to point out (Sluckin 1975) that this and related theories (e.g. Bateson 1973 *op. cit.*) were a development of what I called the perceptual-learning view, implicit in Sluckin & Salzen (1961, in Sluckin 1972) and Bateson (1966 *op. cit.*). This view also goes by the name of exposure learning (Sluckin 1972). Rajecki et al. do not now specifically refer to Salzen's neuronal model; and yet the Bowlby-Ainsworth "ethological theory" described in detail and given approbation in their paper is clearly of the same lineage.

The summaries of the eleven theories of attachment presented in the early part of the article seem to me to be both accurate and succinct. I should like to make just one observation on the section concerned with the Hoffman-Solomon theory. The theory, among other things, sets out to explain the finding that ducklings in strange surroundings make distress calls, but that these cease "when a salient object is introduced." Fresh light has recently been shed on this phenomenon by Brown & Hamilton (1977). In that study imprinted young domestic-fowl chicks in an unfamiliar environment did not approach an attachment figure. A recent series of experiments by me and my colleagues, yet to be submitted for publication, has clearly confirmed that responses to the familiar figure are inhibited by an unfamiliar setting; but this is the case only when the initial imprinting is rather weak; otherwise, a fa-

miliar figure in a strange environment does evoke strong approach responses. More research is needed on the extent of inhibitory power that the unfamiliar environment can exert on the behaviour of infants in the presence of attachment figures. Any successful theory of infantile attachment would have to accommodate the Brown-Hamilton effect.

The authors have wisely, I think, selected no more than four phyletic levels for the examination of findings on infantile attachment. I am tempted to say a few words about the guinea-pig with which my colleagues and I have been concerned for over a decade. These animals do form attachments to animate as well as inanimate figures (Sluckin 1968; Sluckin & Fullerton 1969). We have no evidence as to the effects of maltreatment on the formation of such attachments. Infant guinea-pigs show more locomotion in the presence of the attachment object, which was interpreted as exploratory behaviour (Porter, Berryman, & Fullerton 1973). They also emit particular calls to regain contact with the mother-figure (Berryman 1974), which may perhaps be regarded as "distress" at separation.

The authors remark that the fact that inanimate objects can serve as secure bases is not entirely compatible with the predictions of the "ethological theory." This may be true for some versions of the theory but not for others. Generally, the Bowlby-type view of attachment formation is, as mentioned earlier, akin to the views of imprinting expressed by Bateson, Salzen, and Sluckin. Attachment formation is preference-learning by exposure, and it applies equally to animate and inanimate figures. It is a central feature of that view that, for example, a wobbling box for a chick, or a soft blanket for a human infant, can become objects of attachment, thus providing a secure base for exploration and causing distress if removed.

In my own early accounts of the formation of imprinting attachments I tended to regard the young animal as relatively passive rather than as actively seeking attachment. The term I used – viz. exposure learning – may be thought to imply passivity on the part of the learner. It has become gradually clear, as the authors rightly stress, that the learner, bird or mammal, is active in this learning process. Therefore, the phrase "exposure-learning view" may be somewhat inappropriate to describe the Bowlby-Ainsworth position. However, "ethological theory" seems even more inappropriate, because it may imply that it is close to the views of attachment expressed by Lorenz or Hess. The Bowlby-Ainsworth view, however, is not at all like the Lorenz-Hess position, but it is clearly in the Bateson-Salzen-Sluckin tradition.

Lastly, it seems to me that the Bowlby-type analysis of infantile attachment and Cairns' contiguity analysis are not at all as far apart as it may at first seem. It is to be hoped that a systematic comparative examination of these two types of theorizing will some-day be undertaken with a view to arriving at a possible synthesis. For one must agree with the closing remark of Rajecki et al. that attachment phenomena have not yet been provided with a completely satisfactory explanation.

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**Further implications of opponent-process theory.** The Rajecki et al. paper is a useful contribution to our conceptual grasp of attachment and separation. It deals fairly with the various theories of attachment, and it offers a "secure base" hypothesis that can be tested in a variety of ways.

The opponent-process theory of acquired motivation is, I think, judiciously appraised. The discussion might have been improved with inclusion of more recent developments of the theory. The Hoffman & Solomon (1974 *op. cit.*) and Solomon & Corbit versions (1973, 1974 *op. cit.*) were the earliest and most primitive. The 1977 chapter by Solomon in the Seligman and Maser volume on *Psychopathology: Laboratory Models* would have been a better subject for discussion, for in that chapter the parametric similarities of attachment, on the one hand, and addiction, on the other, were discussed, as were recent experiments on separation. Rajecki et al. might have paid attention, as well, to the unique deductions from the opponent-process theory about the growth of the distress syndrome, because these deductions do not derive from any of the other theoretical positions. The best case in point is the time interval between stimulus exposures, wherein a law of massed practice seems to hold.

The weaknesses of arousal theory were reasonably pointed out. Such a theory is probably the least satisfactory of all the theories we now have.

Rajecki et al. have done us a scholarly service, for their paper is thorough, inclusive, and analytical.

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**Is a general theory of attachment feasible?** The broad comparative analysis of attachment phenomena provided by Rajecki et al. does little to enhance the status of any of the major theories critically reviewed. The authors' overall conclusion is that no current theory of infantile attachment can successfully account for all of the empirical findings in all the species examined. While a few flaws can be found in the presentation of some theoretical material (e.g., in the description of opponent process theory, "processes" and "states" are repeatedly confused), and while proponents of the various theoretical positions could probably provide fairly convincing *post hoc* explanations for some of the findings, the authors' overall conclusions remain intact. There does not presently exist a single theory that can account, *a priori*, for all the phenomena described in the review. It is true that some of the theories provide better fits for certain classes of findings (e.g. separation effects) than do others, and it is probably true that minor modifications in some of the positions might result in accommodation of a greater proportion of the relevant data. Still, these considerations do not invalidate the authors' basic conclusions.

The apparent failure of these theories to account for all of the data does not necessarily imply that they are all "wrong" or otherwise flawed. Rather, a given theory's "fault" may actually only be that it is too limited in scope. It is conceivable that each theory is essentially "correct" and parsimonious for a given set of phenomena, but that different sets have different controlling variables that are best represented by different theoretical positions. Every theory, after all, has some limits to its generality.

For example, among birds, dogs, monkeys, and humans there may be very different ultimate factors contributing to various qualities of mother-infant bonds. Such factors might include modal litter or clutch size, duration of primary maternal dependence, time between pregnancies, and structure of social unit, all of which help determine the form and frequency of social stimulation that any infant will experience during socialization. Thus, an ethologically-oriented theory developed to account for human attachment will typically focus on single infants with protracted periods of infancy in nuclear family settings. Should one expect such a theory to be useful in accounting for differences in imprinting phenomena among precocial birds, which can involve many offspring during very brief periods of time across a variety of settings? Similarly, instrumental conditioning accounts of the development of proximity-seeking and maintaining behavior in human infants usually have difficulty explaining the proximity-seeking and following behavior of chicks during their initial exposure to a mother or mother-substitute. Theories tailored to specific sets of ultimate factors should not necessarily be expected to generalize to species that have been subjected to grossly different selection pressures.

Moreover, any general theory of attachment must address the fact that different species have different sets of proximate factors influencing how an infant becomes bonded to its mother. For example, current data strongly suggest that mother-infant attachment formation in precocial birds is mediated primarily through visual systems (Immelmann 1972) (and/or auditory systems



in some species (Gottlieb 1976), in rats primarily through olfactory cues (Leon 1978), and in monkeys through tactile stimulation (Harlow 1958). Moreover, such different underlying mechanisms could well be governed by different sets of rules.

The above points underscore the difficulty of formulating any general theory of attachment that can successfully account for highly specific data from many different species. It is conceivable that these same points could also be applied to individuals *within* a given species. Nature in her ultimate wisdom has permitted a certain redundancy of function to characterize many aspects of development, so that there may be several different routes by which an individual can reach a given physical and/or social status (Waddington 1966; Bateson 1976). Different situations put different demands on the individual that require different behavioral capabilities. These different behavioral capabilities may have evolved somewhat independently in the same species; if so, expressions of these various capabilities might be controlled by somewhat different mechanisms. Bischof (1975) has recently developed a systems-based model of attachment behavior that incorporates this possibility.

Two other factors may also contribute to the problems encountered by most current attachment theories in addressing all the data for even a single species. First, the theories reviewed do not all share the same level of analysis. For example, in consideration of attachment phenomena there is a major difference between the level of traits utilized by some ethologically-oriented attachment theorists and the level of molecular sequences of behavioral exchanges employed by some social-learning theorists. Similarly, many of the theories reviewed focus on different units of time. Cairns' (1966 *op. cit.*) contiguity-conditioning approach deals with periods of days, weeks, or even months, whereas some S-R theories examine behavioral change in terms of minutes, seconds, or even milliseconds. It is, of course, usually difficult for a theory derived primarily from data at one level of analysis to maintain its predictive accuracy for dependent variables at other levels of analysis.

Secondly, most of the theories reviewed are not truly developmental in nature – i.e., they do not explicitly take into account the accumulating evidence that an infant's physical, perceptual, cognitive, and social capabilities change considerably as it grows older. For example, several of the theories reviewed cannot account for data which demonstrate that some infants can become attached to social objects that consistently mistreat them. They each assume that the infants can associate punishment with its source and thus should avoid (not become attached to) the objects. However, if an infant, when first exposed to the object, is not yet cognitively capable of recognizing that the very object it is clinging to or following is the same one that produces the punishment, attachment might well ensue (Kaufman & Rosenblum 1969). Similarly, the failure to obtain in infants of some species the changes in reaction to repeated separations and reunions predicted by opponent process theory may in part be due to the infants' inability to process the associative cues that the theory incorporates in its analysis of the dynamics of affect change.

Given these considerations, is any general theory of attachment feasible? Such a theory would have to be sensitive to differences in species, living environments, levels of analysis, and developmental status that characterize data in the attachment literature. Perhaps instead of trying to fit these disparate data into a single theory, a more eclectic approach involving a combination of theories, each with sharply-defined limits, would be appropriate. For example, one might begin with a systems model similar to that developed by Bischof (1975), in which different behavioral "systems" are activated by different environmental events via different sets of mechanisms. By incorporating such systems into a hierarchical model, different levels of analysis could be accommodated; by allowing the hierarchy to emerge throughout ontogeny à la Piaget, developmentally-based behavioral changes could be addressed. Of course, such a model would hardly be parsimonious, and it might very well be untestable, at least with respect to existing data. Nevertheless, it does represent one alternative approach toward developing a general theory of attachment.

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**Detaching from attachment.** The experimental analysis of behavior has identified a number of environmental manipulations capable of modifying infantile attachment. The terms "attachment" and "social bonding" have been applied so ubiquitously that it is no longer possible to distinguish attachment behavior from social development in general, and it is no longer evident that attachment behavior refers to a discrete set of phenomena requiring its own theory. Rajecki et al.'s article provides a valuable service by summarizing comparative-experimental data on infantile attachment, sorting out the phenomena from which social bonding is usually deduced, and examining the concordance between experimental findings and contemporary theories of attachment. However, the article does not propose a general theory of infantile attachment, and it does not examine the conceptual errors and methodological shortcomings that stand in the way of formulating such a theory. Nor does it present a comprehensive review. Potentially relevant studies on early socialization that fall outside the tradition of experimental psychology are not reviewed (see, for example, Bernal 1974; Dimond 1970; Impekovan 1976; Poirier 1972; Rosenblatt 1976; Spencer-Booth 1970).

As the authors suggest, any adequate theory of infantile attachment must appreciate the particular adaptive requirements facing the infant as adult. However, if different patterns of early socialization are optimal for diverse developmental outcomes, the essential topic of inquiry is no longer the intensity of attachment, but the relation between qualitative variations of social bonding in infancy and capacity for adaptation to different species- or culture-specific adult environments. This point seems crucial for any general theory, but it is not explored in any depth. Instead, the article summarizes studies which measure outcome only in terms of the infant's attachment behavior during infancy. Experimental studies and clinical observations that evaluate the functional significance of early socialization for long-term outcome are not considered (see, for example, Dunn 1976, 1977; Rutter 1972; Tizard & Rees 1974, 1975). Yet, one might conclude from such studies that the effects of early socialization on adult characteristics will not be direct, as a critical period hypothesis would predict, but will be modified continuously by intervening physiological changes and social experience.

Rajecki et al.'s review focuses on the experimental manipulation of social-psychological variables that influence attachment in the infant. There is, however, a growing body of evidence to suggest that socialization depends on a complex interaction of neuro-endocrine, situational, and experiential factors which operate on the social partner as well as the infant. A number of studies are cited which suggest that infants will become attached despite, rather than because of, the social partner's responses, and that transactions between infant and parent or surrogate are of little relevance. There is no mention of other studies indicating that the infant's behavior can significantly influence and be influenced by the partner's physiological and social responses (see, for example, Cairns 1972; Rosenblatt 1967, 1970). There is also no mention of studies demonstrating that early social experience (i.e. "attachment behavior") is only the first and not necessarily the crucial determinant in a developing network of social relationships with siblings or littermates, adolescents, and other adults, which may differ in kind or degree, depending on the species and culture (see, for example, Bateson 1973; Hinde 1971a, 1971b; Hinde & Spencer-Booth 1969; Schaffer 1977).

Since the consequences of early socialization for adaptive fitness are of

greater importance for a developmental or evolutionary theory of socialization than the mere presence or absence of infantile attachment, appropriate methods for analyzing the qualitative aspects of social bonding may also be more relevant than quantitative measures of attachment behavior. The experimental methods summarized in Rajecki et al.'s review deduce causal relations from frequency counts and the statistical correlation of contiguous events. However, entirely different mathematical models may be required to analyze the qualitative characteristics or "syntactic structures" of the bonding process (Wolff 1972).

The substance of the review suggests that long-term developmental consequences are not of great interest for contemporary students of infantile attachment. Yet a concern with the functional relation, if any, between early social experience and long-term adaptive fitness is central to any biologically or psychologically meaningful theory of socialization. The issue cannot be ignored as long as social engineers extrapolate from the experimental data on attachment behavior to formulate programs of infant enrichment that ignore cultural variations in adaptive requirements, and as long as experts use such data as a basis for prescribing child-rearing practices to the population at large.

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## Authors' Response

by D. W. Rajecki and Michael E. Lamb

### Interpretations, reinterpretations, and alleged misinterpretations of theory and data concerning attachment

Social scientists have long adopted an ambivalent stance regarding behavioral comparisons across species. As Harlow, Gluck, and Suomi (1972) wrote: "Basically, the problems of generalization of behavioral data between species are simple – one cannot generalize, but one must. If the competent do not wish to generalize, the incompetent will fill the field." Several of the commentators chose to question the correctness of our comparative efforts, while others argued that such comparisons were essentially inadmissible. Inadmissibility would surely be the most severe charge against a paper providing "a comparative review of aspects of the social bond." To attempt something that cannot be done must rank as the ultimate form of incompetence.

One potential solution to the generalization problem is to ignore all species except the one that is the most interesting. This has been the approach of social psychologists; for many, non-human animals are effectively invisible (Rajecki 1977 *op. cit.*). Alternately, one could develop separate disciplines (or subdisciplines) for each species. This seems to be the *modus vivendi* in contemporary comparative psychology. Although sympathetic to the interests of colleagues, researchers tend to specialize – coming to define themselves as "rat runners," "bird brains," or "monkey men" (cf. Harlow et al. 1972).

These traditional approaches must be scrutinized in the light of recent developments – notably, the ascendancy of ethology and sociobiology. Like it or not, comparative, developmental, and social psychologists must face the fact that humans are animals, and that the biology and psychology of our species doubtless bear lawful relationships to those of all other animals. Although our paper did not provide an ethological or sociobiological analysis, its focus was related to principles central to those disciplines.

Given our implicit interest in analogy, we were remiss in failing to deal explicitly with the distinction between analogy and homology in comparisons across species. Our failure to do so led Brown, Eble, Immelman, and Kaufman (among others) to question our choice of species. Clearly, one's choice is directly dependent on the nature of one's aims. If one is interested in the direct solution of problems within contemporary human society, then one should limit oneself to the study of humans. On the other hand, if one wishes to consider the biology and evolutionary history of our species, then additional options are available, although strict rules of choice are pertinent – specifically, those rules pertaining to the search for homologies. A homology is a physical or behavioral similarity between nonconspecifics that is genetically-based and can be traced to a common ancestor. On the basis of morphological taxonomies some of the great apes appear to be quite closely related to humans (Hodos & Campbell 1969). Washburn (1978) has reviewed the research on molecular biochemistry and concluded that humans are tightly grouped with chimpanzees and gorillas. Consequently, there is probably sufficient genetic similarity between humans and these apes to

warrant the search for homology. Unfortunately, this approach is not free of obstacles. Atz (1970) has argued persuasively that the search for physical or structural homologies can be aided by criteria concerning position in comparable systems of structures, special or unusual quality, and constancy or continuity in intermediate form, but that these principles are very difficult to apply when behaviors are being compared. After surveying the difficulties, Atz concluded that "the essentially morphological concept of homology cannot at present be applied to behavior in any meaningful (nontrite) way because of its lack of structural correlates." Thus, attachment theorists interested only in homologies to human behavior are in an unenviable position: they may legitimately study only a few relatively inaccessible species, and clear guidelines regarding the inferences they may reach have yet to be developed.

If the biological basis of "human nature" were our sole concern, furthermore, there would be no reason to study the behavior of precocial birds, puppies, or monkeys, since none of these species is in the line of human evolutionary descent (Hodos & Campbell 1969). Fortunately, homologies do not represent the only legitimate comparisons. One can search for analogies reflecting general principles of adaptation and survival [cf. Eibl-Eibesfeldt, "Human Ethology," *BBS* 2(1) 1979]. When environments place similar intense demands on even the most genetically unrelated species, these species (via convergent evolution) may develop similar physical or behavioral characteristics in adapting to those conditions. To permit efficient movement through water, for example, dolphins and sharks have roughly the same outer shape, although they are not closely related species (Lorenz 1974).

The rules for drawing behavioral analogies are not as strict as those regarding homologies. In fact, the only rule appears to be that common sense prevail (Hodos & Campbell 1969; Lockard 1971; Lorenz 1974). In our search for analogy we are aware that the species-specific attachment behavior of chicks or ducklings tells us nothing about the species-specific behavior of any other species under review. The behavior of all these animals, however, may illuminate the concept of attachment and its adaptive significance. In this endeavor attachment need not be seen as a unitary process, a set of responses, or an end product. It is a concept similar to the notions of territoriality, dominance, migration, dispersion, communication, and learning. These concepts (and many others like them) are useful, not because they permit predictions from one phyletic branch to another, but because they provide a framework within which to organize and understand the variability in animal behavior.

**Alleged misinterpretations: classical conditioning theory.** We wish to turn now from considering the comments of those who question the legitimacy and potential usefulness of reviews such as ours to the comments of those who question the quality of the review itself. Hoffman's comments are especially critical. He seeks to undermine the review by arguing that his classical-conditioning model "has never asserted that [social] attachments form," whereas we evaluate its ability to account for phenomena related to the *formation* of social bonds. Clearly, Hoffman's intention is to portray us as confused. Regrettably, the confusion is wholly attributable to Hoffman. If one examines the articles in which Hoffman elaborates on the conditioning model (Hoffman & DePaulo 1977; Hoffman & Ratner 1973, both *op. cit.*), one finds key passages replete with unqualified assertions that the formation of bonds is at issue. For example, Hoffman and DePaulo write:

"Like many other precocial birds, newly hatched ducklings will follow and *become socially attached* to the first moving object they encounter. . . . Moreover, rather than being unique to precocial birds as is often supposed, the behavioral processes of the imprinting phenomenon appear to operate in the *formation of social attachments* in many of the higher animals, including primates and man" (italics added) (Hoffman & DePaulo 1977, p. 58).

Elsewhere, Hoffman and Ratner state that:

"It is another issue, however, as to whether the learning of the details of a given stimulus configuration (such as the rearing environment) necessarily reflects the *formation of a social bond* between the bird and that stimulus configuration; the perceptual learning interpretation of imprinting assumes that it does. However, a recent study in our laboratory . . . has indicated that such learning is not sufficient for the *formation of a social attachment* by an immature precocial bird" (italics added) (Hoffman & Ratner 1973, p. 529).

Finally, Hoffman and Ratner stress that: "While laboratory studies (of imprinting) have described the processes involved in the *formation of a social bond* and in the termination of the critical period, they have also demonstrated that *new bonds can be formed* after the critical period has ended. Although partly a semantic distinction, several investigators have argued on this basis that the supposed 'critical period' for imprinting is really only a 'sensitive period.' As discussed earlier, however, this assertion is based on the investigator's ignoring (sic) the effect of conditions that exist in a natural setting that preclude the *development of attachments* subsequent to the critical period. . . . (italics added) (Hoffman & Ratner 1973, p. 541).

Ironically, Hoffman's claim that his theory "has never asserted that such attachments form" represents yet another reason to be skeptical about his theory's utility in future research and thinking concerning attachment. It is significant that Hoffman does not respond to any of the criticisms raised in our review of his classical conditioning theory.

In his comment Hoffman also accuses us of withholding information about Ratner's (1976 *op. cit.*) research on the influence of aversive stimulation on imprinting. The finding at issue is that, at one point in testing, ducklings preferred a shock-associated object over an object that was highly novel (see Ratner 1976, Figure 3). Concerning this information, Hoffman states that: ". . . Rajecki et al. failed to report that this effect was only obtained in the initial test with these subjects, and that with continued testing the birds came to exhibit an overwhelming preference for the novel stimulus. In short, as in the case with Rajecki et al.'s treatment of theory, their interpretation and judgment of data also appear to have been strongly influenced by their personal views."

Hoffman's criticism can be faulted on two grounds. First, the ducklings' reactions in the initial test cannot be ignored because they later behaved differently. What transpired in later testing could not have influenced what happened in the first test. Second, Hoffman's contention concerning the "overwhelming preference for the novel stimulus" is at best misleading. Preference for this "novel" object was observed only after the birds had 20-minute exposure to the thing in the absence of the object to which they had been exposed originally. The stimulus was clearly not novel. It is well known that filial imprinting is reversible (Salzen & Meyer 1967), and that preferences for new objects can be established with far less than 20 minutes of exposure (Eiserer & Hoffman 1974). A shift in preference does not mean that the ducklings were not previously imprinted on the original shock-associated object.

Eiserer proposes that several predictions of the classical conditioning model are derived from a principle that is not one of the model's five basic premises. Inasmuch as these premises alone comprise the classical conditioning theory, he argues, the theory remains unassailed. We are not persuaded by this argument, for the predictions concerned are derived directly from Hoffman's own contributions, and there is, in any event, good reason to question the classical conditioning explanation of imprinting/attachment. Hoffman and Ratner (1973 *op. cit.*), for example, state that ". . . if, as suggested here, imprinting stimuli are innately reinforcing, it would be expected that all of the behavioral effects usually ascribed to primary reinforcing stimuli should be part of the imprinting phenomenon." This is not the case in practice. One of the behavioral effects ascribed to primary rein-



forcing stimuli is that, upon their removal, extinction occurs. Thus if the movement of an imprinting target (the UCS) is eliminated, the model predicts that the learned responses to its static features (the CS) will extinguish. Eiserer, Hoffman, and Klein (1975 *op. cit.*, Experiment I) found (predictably) that the static features did not initially suppress ducklings' distress calls but did so after a total of 160 minutes of association with the movement of the object. However, extinction did not occur. When in the absence of movement, the static features were presented, for a total of 600 minutes over a 10-day period, these stimuli lost none of their suppressive influence (see also Eiserer et al. 1975 *op. cit.*, Experiment II).

**Alleged misinterpretations: opponent process theory.** Mineka & Rush accuse us of acting in a disingenuous manner. They contend that we have mauled the opponent-process theory beyond recognition and therefore are not adequate to be judges of it. Hoffman echoes their claim. (Suomi also complains about a lack of distinction between states and processes.) Fortunately, however, not all those involved in the construction or defense of the theory perceive egregious misstatements on our part. Solomon, in fact, describes our appraisal as "judicious." Indeed, in reaction to the assertion that we confuse states with processes, we must ask: So what? Although many writers in this area treat processes and states as separable intervening variables, states and processes have (to our knowledge) never been measured independently. What is the operational definition of an *a-* or *b-process*? After determining when an *A-something* (or would it be an *a-something*) is going on in an organism, what more can a researcher say?

**Alleged misinterpretations: operant learning theory.** For the most part, Mineka & Rush do not dispute our conclusions regarding the inadequacies of the operant learning theory. They defend the theory by arguing that we should have criticized the ethological theory more strongly – thereby acknowledging, we presume, the flaws identified in the operant learning approach.

Somewhat curiously, meanwhile, Passman & Adams postulate the existence of numerous innately-reinforcing stimuli in their defense of operant theory. This is, of course, an appealing and easy strategy to adopt *post hoc*. Unfortunately, the theory thus modified remains unable to account for the maltreatment phenomena that have been documented. The theory cannot explain why chicks would show enhanced attachment to an object that has previously struck them (Salzen 1970 *op. cit.*) or pummeled them severely (Rajecki et al., in preparation, *op. cit.*). The same data invalidate Passman & Adams' defense of Hoffman's classical conditioning theory.

Where secure base phenomena are concerned, Passman & Adams simply assert the adequacy of both the operant and classical learning theories, but they fail to substantiate the assertion and thus leave our arguments unchallenged. Finally, in their discussion of separation effects they confuse multiple and extended separations. Their hypothesis seeks to explain the decrement in protest following repeated separations, but it cannot account for the fact that over the course of a given extended separation the amount of protest decreases rather than increases (Cairns 1966 *op. cit.*).

There is an additional deficiency in Gewirtz' operant learning theory not mentioned in our original review. Since, in his view, the infant's attachment responses are under the positive stimulus control of the object, removal of the object (separation) should result in the cessation or disruption of those responses. In fact, however, the infant does not merely stop engaging in one class of activities; instead, it is likely to exhibit vivid reactions variously described as reflections of "protest," "agitation," or "disturbance."

Gewirtz' theory cannot readily account for such behavior. In several places Gewirtz (1972 *op. cit.*, see, for example, p. 153) suggests that "interference with response sequences" would lead to frustration, which in turn would be "followed frequently

by intense emotional responses," but the intensity of the reaction is problematic. Certainly an infant that was greatly desirous of social stimulation or interaction might experience an intense reaction if avenues to those goals were blocked. But infants do not incessantly crave either direct social contact (touching) or interaction (communicating), for they routinely depart from the attachment object on their own initiative, and their forays often take them out of sight of that object (see Rheingold & Eckerman 1970 *op. cit.*). Since the infant's desire for social stimuli can vary, this reinforcement analysis must specify how separation reactions and this variability are related.

The simplest prediction from reinforcement theory is that the desire for social stimuli should be a positive function of the length of separation (or deprivation). Gewirtz (Gewirtz & Baer 1958) himself has shown that, for school-age children, social reinforcement (praise) is more effective after even a short period of segregation. Thus a separated infant should experience the least desire for social stimuli immediately after the removal of the social object, because it is at least partly satisfied by the recent availability of social reinforcers. Over time the infant's desire should increase, and, if anything, emotional reactions should also increase.

Unfortunately, this does not happen. For example, Cairns (1966 *op. cit.*) has convincingly demonstrated that emotional reactions in lambs (bleats per minute) are most intense immediately after separation, and ebb thereafter. It is incumbent upon theorists such as Gewirtz to explain why this is so. In the absence of such an explanation there exists yet another reason to fault the operant approach.

**Alleged misinterpretations: ethologically-oriented theories.** In evaluating the ethologically-oriented theories, we were forced to question the prediction that infants become attached to individuals (a) who respond appropriately and consistently to the infant's signals, and (b) with whom the infant has had a "sufficient" amount of interaction. Several of the commentators (Ainsworth, Blanchard, Main\*, Bischof, Gibson, Chalmers, Kovach) correctly pointed out that the prediction concerning appropriate responsiveness derives from the writings of one of the present authors (Lamb), and that its invalidation does not necessarily negate any central tenets of the ethologically-oriented theories. These commentators also argued convincingly that the occurrence of attachments to "inappropriate objects" does not invalidate the theories' presumptions since, in Main's words, "natural selection only needs to develop a means of identifying the prospective attachment figure(s) likely to distinguish her (them) from others." According to this view, it is not surprising that one can find (or construct) situations in which an object that appears inappropriate nevertheless evinces the crucial characteristics and is hence selected as an attachment figure.

Where humans are concerned, however, the ethologically-oriented theorists have failed to specify the crucial characteristics by means of which infants identify and select among potential attachment figures. As we argued in our review, the ethologically-oriented theories appear to be the most viable of those reviewed. In several respects, however, the set of notions we have labelled "an ethologically-oriented theory" could be described as an *orientation* rather than a *theory* (see also Chalmers' commentary). The merit of a theory ultimately depends on its ability to pose questions and to predict phenomena: an orientation that permits *post hoc* explanation must be formalized if it is to become a falsifiable theory.

**Concerning the effects of maltreatment.** The effects of maltreatment remain poorly understood, although several commentators reflected thoughtfully on this issue. Some (e.g. Blanchard) argued that maltreatment by adults is in fact fairly common in infancy. There are several directions in which research on maltreatment could profitably proceed. In our review, for example, we stated that it would be useful to have more information about the emotions involved in attachment. In this

connection, Rajecki et al. in preparation, *op. cit.*) found that chicks reacted in a complicated manner to beatings from their imprinting object. At the instant they were struck they invariably emitted a trill (sine wave note), which was typically followed by many distress vocalizations (descending notes) in the period between blows. Interestingly, however, the observers heard contentment calls or pleasure notes (ascending notes) interspersed between the trills and the distress calls, and spectrographic analyses of the audiotaped records confirmed the observers' impressions. These data suggest that even chicks react ambivalently to abuse. Detailed analyses of reactions to maltreatment may advance understanding of reactions to imprinting objects in other contexts.

**The problem of specific attachments.** An important issue is raised by Gunnar, who rightly feels that it would be pointless to try to study attachment in infants that did not form attachments. We disagree, however, with Gunnar's contention that neither puppies nor chicks form specific attachments. Any pet owner will dispute Gunnar's claim that young dogs do not form specific ties, though the scientific evidence is lacking. With respect to chicks, there are two types of evidence demonstrating that these birds form bonds with specific individuals. First, the immense literature on imprinting indicates that chicks unquestionably become psychologically attached to something. Second, chicks make remarkable social discriminations on the basis of extremely little experience. After as little as 16 hours of cohabitation, day-old chicks peck more at (i.e. discriminate between) non-cagemates than at cagemates (Rajecki, Ivins, & Rein 1976; Zajonc, Wilson, & Rajecki 1975). Naturalistic observations further suggest that chicks discriminate between their dam and other hens, although these observations are not highly detailed. McBride, Parer, & Foenander (1969) report that in the wild the broods of junglefowl hens intermingled at feeding sites, but that the dams "gathered their broods by tidbitting (a form of social display), which attracts the chicks" (p. 139). Presumably, those chicks distinguished the tidbitting of their dam from that of other adults.

The evidence, therefore, does not support the argument that chicks are not suitable vehicles for the study of attachment. Before any species is included or written off, it might be fruitful to study their infants in the manner exemplified by Coe, Mendotz, Smotherman, and Levine (1978). These researchers found elevation in the plasma cortisol levels of infant monkeys separated from their mothers, even when those infants were "aunted" (carried by some other adult female). This is strong evidence for the existence of specific attachments. One wonders what comparable tests with puppies would show.

Such studies might also alleviate Brown's exasperation with our statements about *reliable* findings that monkeys are strongly influenced by involuntary separation. Brown suggests that "bonnets simply hop onto another adult in the family group." The report of Coe et al. (1978) and the comments of Dolhinow suggest that such a hop may be neither simple nor satisfactory. In a pertinent study Kaufman and Stynes (1978) found that the social and solitary play of bonnet infants was greatly reduced when their mothers were removed, even when other bonnet females were present. Moreover, severe depression was induced in a bonnet male when all bonnet adults were removed from the rearing unit. This infant had access to familiar pigtail adults and infants, but after three days of protest (agitation) its posture collapsed and it engaged in huddling behavior.

**Sins of omission and commission.** A number of commentators, including Hess, Salzen, Scott, Sluckin, Solomon, and Cairns, submit that we have omitted some significant articles, and in this they are correct. Fortunately, the record is clarified by the content of these commentaries. Furthermore, in considering the material that was omitted – intentionally or inadvertently – we

are not persuaded that its inclusion<sup>1</sup> would substantially change the conclusion of the review. If the commentators judge otherwise, it is incumbent on them to show why. Of course, if we have maligned or underrated certain contributors to this area – most notably Hess, Lorenz, and Schneirla – we can only extend our apologies.

**Conclusion.** Our review was designed to clarify and identify issues in need of research and to prompt commentaries that would serve this goal. Several of the commentators provided clarifications, alternative perspectives, or empirical data not previously available, and we are delighted to have provoked these contributions to the field. Especially useful, or provocative, in our opinion, are the comments of Ainsworth, Cairns, Kovach, Masters, Rutter, Suomi. If nothing else, these comments demonstrate that whatever deficiencies we uncovered in the theories reviewed, there remains sufficient interest in the topic to ensure that the theories will be elaborated, reevaluated, and improved in the years ahead. We hope that the theorists of the future will learn from the difficulties of their predecessors – this, surely, is how scientific advance is forged.

#### NOTE

1. Had we known about Salzen's recent elaboration of his neuronal modeling theory (Salzen, E. A. Social attachment and a sense of security – a review. *Social Science Information*. 17:1978 in press) at the time of writing, we would have included it in the review. However, in light of the possibility that the Salzen paper may not be widely available as yet, we prefer to take up that work at some later time in the Continuing Commentary section of this Journal.

#### EDITORIAL NOTE

\*To appear in forthcoming Continuing Commentary.

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